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The Motivational Concept in Behavior Theory

Judson Seise Brown

The Motivation of Behavior. New York: McGraw-Hill, 1961. Pp. xii + 404. $7.50.

Reviewed by LAWRENCE I. O'KELLY

Judson Brown, now bearing the distinguished title of Graduate Professor of Psychology at the University of Florida, received the doctorate at Yale in time to carry it with him to Harvard for a year, and not necessarily through demotion, back to Yale for an equally brief period of time before he began, as an aviation psychologist, to fight World War II. After the war he went to Iowa State where, productively, he remained until 1959. Southerners will note that it took only two years in Florida for him to produce the present book. Lawrence O'Kelly, the reviewer, entered psychology through Ohio State University, and like, but not with Dr. Brown, served as a psychologist in World War II. He describes this wartime period as a "forced interlude as a clinical psychologist." Before and after the war he was at the University of Colorado, then later at the University of Illinois, where he now continues his research and publication in physiological psychology. He has focused his curiosity on physiological processes of regulation as they relate to behavior and presently is concentrating on water metabolism and thirst-motivated behavior.

—F. H. S.

An adjective that appropriately characterizes Professor Brown's book is one that might seem strangely unnecessary in a journal devoted to the appraisal of scientific publications, and the reviewer was surprised to find how persistently the impression of "honesty" occurred to him as he studied and taught from this text. But in this day of competition for gains in textbook sales there are many treatments of complex psychological questions that sacrifice just a little objectivity for greater popular appeal, that elegantly simplify by omitting complicating evidence, that seek to meet all needs and to disagree with no one. None of these censorious tendencies are true of *The Motivation of Behavior.* It is an *honest* book, acknowledging difficulties, presenting alternative points of view, making its own position completely clear where possible, and advising withheld judgment when necessary, recognizing its own limitations. Speaking from a semester's experience in teaching it, the reviewer can say that it stands up well to the scrutiny of student and teacher alike.

Brown's purpose in writing the book goes a great way towards determining its assets and liabilities. When a person sets out ". . . to develop a tightly reasoned, systematic analysis of the concept of motivation, with special emphasis upon its relative utility as an explanatory component of general behavior theory" he must soon face the difficulty of defining "motivation" in a fashion that reduces its manic comprehensiveness to manageable proportions, and do so in a manner that will provide a strategically sufficient body of verifying empirical evidence. To those who have followed Professor Brown's experimental and theoretical contributions to the field, there is no surprise in his choice of Hull's multiplicative-drive theory as the basis for his own analysis of motivation. In Brown's hands, and profiting by Spence's significant analysis of aversive and incentive motivation, the drive construct has impressive explanatory force. It is somewhat paradoxical, however, that defining drive *(D)* in such a fashion as to eliminate all but the energizing function in behavior, qualitatively different motivational states disappear, and almost the whole burden of explaining the quality or direction of behavior is placed on the unlearned and learned reaction tendencies, certainly a strong concession to associative theories of motivation. Brown himself, at various places in the book, and most particularly in his Chapter 4, "Motivational and Associative Interpretations of 'Motivated' Behavior," seems close to being convinced that exclusively associative explanations may, after all, be sufficient.

By defining *D* as he does, then, Brown finds it possible to write a text on motivation that has almost no organized content relating to the directional aspects of behavior. Thirst, hunger, pain-avoidance, sexual behavior, and individual and group characteristics of

human behavior are not treated comprehensively, but only indirectly and incidentally as studies in these fields supply appropriate evidence for some aspect of drive or drive-habit interaction. Even so, it is surprising how much information on specific drive states does emerge in the course of his analysis.

The book is well organized for teaching. Leaving little to chance previous contacts with formal behavior theory, Brown devotes the first two chapters to systematic definition of concepts, an illuminating discussion of theory construction in general and of specific problems of theory in motivation, and to the logic of measurement. These chapters are very well written, and were almost universally intelligible to a very assorted class of 50 students who ranged from borderline juniors to advanced graduate students. A chapter on primary sources of drive is perhaps the weakest in the book, for reasons to be discussed later. The next five chapters are the heart of the book. After posing the persistent problem of motivational versus associational interpretations, he discusses learned responses as sources of drive, motivational consequences of frustration and conflict, motivational variables in human performance, and motivational variables in perception. The book ends with short discussions of such miscellaneous motivational problems as the conditioning of emotional responses, the "exploratory" drive, physiological "activation" and intracranial self-reinforcement. The writing throughout is refreshingly crisp and clear, illustrations are confined to text-oriented graphic presentations of data, and each chapter has a summary that genuinely summarizes.

Thus the assets and liabilities. On the credit side, a reasonably comprehensive and rigorous treatment of the energizing function in behavior supplies the student with a very high-level exercise in the actual spade work of psychological theory construction. Against this is the quite serious neglect of a broad spectrum of varieties of animal and human motivation. The reviewer found, using the book in a one-semester course, that rather more than a desirable amount of supplementary reading assignments and lecture material was necessary. It is a tribute to the systematic excellence of Brown's treatment, however, that the supplementary material could, for the most part, be integrated smoothly with the chapter topics of the text.

Judson S. Brown

As the foregoing remarks indicate, the reviewer has a strongly favorable impression of *The Motivation of Behavior*. On one major difference in point of view, however, some comment is irresistible. Professor Brown, along with many other American psychologists, insists that behavioral data are of such a nature as to place psychology in a unique position among the sciences. "The psychologist concentrates his attention primarily upon the *behavior* of living organisms, not upon their physiological functions, their anatomical structure, or their physical-chemical composition. Psychological data are thus derived from one class of events and the data of other sciences from different classes. Because of this one may maintain, with some justification, that the subject matter of psychology is unique in comparison with the subject matter of other disciplines" (p. 2).

Although Brown then goes on to mention the possibility and value of interpreting the empirical results of behavior study by reference to known facts concerning an organism's anatomical and physiological makeup, and suggests that "there are many reasons for believing . . . that in the future our explanations of complex human and animal behavior will be considerably broadened by the identification of the neurophysiological and physiochemical systems whose functions make that behavior possible" (p. 22), it is obvious that his preference is for a study of motivation within the network of behavioral level constructs and their supporting empirical laws. While it is true that the "physiological," or more generally, the *biological* approach to behavior, and the "behavioral" approach are supplementary rather than contradictory, it seems to the reviewer that the study of motivational phenomena needs both sets of facts and hypotheses to arrive at an approximation of basic principles.

At no place in behavioral science is the importance of evolutionary principles so immediately obvious as in the area of motivation. The transition from physical and biochemical to behavioral criteria of survival is centered in the relation between the internal, relatively isolated homeostatic and the external, broadly organized behavioral mechanisms. In an environment with heterogenous conditions and distributions of materials, survival is dependent upon behavioral as well as upon homeostatic sufficiency. Making both possible are the selected and evolved structures of the organism. Thus it appears to this reviewer that a consideration of evolutionary and comparative aspects of physiological regulation are as indispensable ingredients of the scientific description of motivational phenomena as are the empirical facts of behavior themselves.

At one time, not so long ago, it could have been argued that the biological approach was correct in principle perhaps, but that available knowledge was so inadequate as to render it premature and inexpedient. This is no longer true; while much remains obscure, the rich store of observations of subcortical processes in primary regulatory activity, the new insights stemming from research on the diffuse projection systems, the growing understanding of biochemical substrates of regulation, the increased utility of genetic concepts in evolution, the greater scope of behavioral data on a wide range of species fur-

nished by the ethologists all certainly provide almost too great a wealth of facts on motivational phenomena for the systematist in this field.

These remarks should not be construed as an attempt to minimize Professor Brown's contribution. He has done impressively and honestly what he set out to do, and the result is a performance that will remain useful for many years. But it would be a sad by-product of such an excellent work if it influenced a future generation of graduate students to neglect the biological foundations of their science. Ultimately all distinctions between "sciences" are temporary artifacts of administrative convenience, stage of development, and human limitation.

Of What Does Consistency Consist?

Milton J. Rosenberg, Carl I Hovland, William J. McGuire, Robert P. Abelson, and Jack W. Brehm

Attitude Organization and Change: An Analysis of Consistency Among Attitude Components. New Haven: Yale University Press, 1960. Pp. xii + 239. $5.00.

Reviewed by DORWIN CARTWRIGHT

Carl I Hovland, the inspiration for this series of monographs, died on 16 April 1961. The fourth volume of the series, Social Judgment: An Assimilation-Contrast Approach to Attitudes and the Effects of Communication, *in which Hovland was joined by Muzafer Sherif, will appear shortly. Hovland was Sterling Professor of Psychology at Yale, Director of Graduate Studies, and a Fellow of Silliman College. Rosenberg has just become Associate Professor of Psychology in charge of the program in social psychology at Ohio State University. He is a Michigan PhD, at Yale in 1954-1961. McGuire has just become Associate Professor in the doctoral program in social psychology at Columbia. He is a Yale PhD, who went to Minnesota, returned to Yale, and then went to Illinois to work in communications. Abelson, a Princeton PhD, has been at Yale since 1953, where he is now Associate Professor. Brehm, a Minnesota PhD, went to Yale for three years and is now Assistant Professor of Psychology at Duke. The reviewer, Cartwright, is Professor of Psychology and Research Coordinator in the Research Center for Group Dynamics in the University of Michigan. He wrote this review while Fellow at the Center for Advanced Study in the Behavioral Sciences. His interest in attitudes and communication goes back to World War II, when he worked under the supervision of Rensis Likert. He was co-editor of* Public Opinion and Propaganda *(Dryden Press, 1954). With Frank Harary he has published a mathematical formalization of Fritz Heider's theory of balance.*

—E.G.B.

IN 1957 the American Psychological Association presented its Distinguished Scientific Contribution Award to Carl I Hovland "for his original and provocative contributions to the scientific study of persuasive communications and the modification of beliefs and attitudes." The book under review is the fourth major publication from Hovland's program of research on communication and attitude change. It conforms to the high standards set by the previous work and makes vivid the great loss to psychology occasioned by his recent death.

Hovland's program has consistently represented the best in collaborative research. He attracted psychologists deeply committed to a variety of theoretical orientations and encouraged each to follow his own theoretical bent. Each investigation, therefore, has been designed to contribute to the solution of a particular theoretical problem. Despite the theoretical diversity of the collaborators, a strong coherence has been achieved by subordinating the theories to an empirical problem; they are made to serve, never to dominate. The product has been a focused body of knowledge with theoretical significance rather than a single, tightly knit theory. This dual allegiance to the empirical and theoretical may also account for the program's markedly practical flavor. No giant steps are needed to get from the research findings to application.

The research reported in this book is directed toward illuminating the interrelation and interaction among the cognitive, affective, and behavioral components of attitudes. How are attitudes organized, and how does organization affect the processes involved in attitude change? The focal concept throughout the several studies is "response consistency." The basic postulate is that man has a "preference for consistency" which manifests itself in many ways in the organization and change of attitudes.

More specifically, three conceptual models serve to generate the hypotheses of the separate investigations. Brehm, employing Festinger's dissonance theory, assumes that the cognitive and affective components of an attitude toward an object imply a certain kind of behavior with respect to that object. He defines "discrepant behavior" as either the absence or the opposite of the implied behavior. Now, if an individual is subjected to forces sufficiently strong to override an attitude and to produce discrepant behavior, a state of dissonance results. The magnitude of dissonance is assumed to be an inverse function of the magnitude of the forces favoring the discrepant behavior and a direct function of the forces opposing it. Thus, if a person is induced to do something contrary to an attitude, he will experience less dissonance the greater the inducement, and more dissonance the stronger the attitude. The "preference for consistency," mentioned above, ap-

pears in this theory as "avoidance of dissonance."

Rosenberg and Abelson, building on the writings of Heider, concentrate on consistency within cognitive structures. The basic units of cognitive structure are "bands" composed of pairs of cognitive elements connected by a term defining their relationship. Both elements and relationships are said to have signs, reflecting the affective quality of the elements and a less clearly specified property of the relationships. A band is said to be "balanced" when the sign of the relationship is consistent with the sign of the related elements; otherwise it is imbalanced. Consistency means here that two concepts of identical sign are positively related, or two concepts of opposite sign are negatively related. It is assumed that a person seeks to avoid imbalanced structures. Thus, the statement "segregated housing is incompatible with the brotherhood of man" would constitute an imbalanced band if the person has positive affect for both "segregated housing" and "the brotherhood of man," since the relationship "is incompatible with" is assumed to be negative. This band could readily become balanced by changing the sign of just one of the elements or by creating a positive relationship between them.

McGuire is concerned with much the same kind of phenomena. His basic elements are "propositions" which may be connected by the relations of formal logic, as in the following syllogism:

(a) A major nuclear war would result in violent death to at least half the earth's population.

(b) A major nuclear war will occur within the next ten years.

(c) At least half the earth's population will meet violent death within the next ten years.

Two types of consistency are specified: "wishful thinking," or the tendency for the subjective probability of a proposition to correspond to its desirability, and "logical thinking," or the tendency for a person's beliefs on related issues to be in accord with each other in a pattern required by the rules of formal logic. McGuire constructs a formal model from which it follows that if *a* and *b* are propositions which together imply *c*, and if *a* and *b* refer to independent events, then the logically required probability of the conclusion is equal to, or greater than, the product of the probabilities of the premises: $p'(c) \geq p(a)p(b)$. If two credible premises imply an undesirable conclusion, the two types of consistency create conflicting tendencies, for 'logically' the person should place credence in the conclusion while 'wishfully' he should not.

ALTHOUGH no systematic comparison of these three conceptual schemes is presented in the book, the similarities among them are tantalizing. The "bands" of Rosenberg and Abelson correspond closely to the "propositions" of McGuire. All three systems employ the notion of "relations among elements": logical implication (McGuire), a more loosely specified implication from cognition and affect to behavior (Brehm), and highly abstract "signed relationships" among cognitive elements (Rosenberg and Abelson). And all three posit a tendency toward consistency. It would seem that the models of McGuire and of Rosenberg and Abelson could be amalgamated rather easily, for each is stated in a fairly rigorous fashion. Moreover, since Rosenberg and Abelson found in their research that subjects favor cognitive structures that maximize prospective gain, they end up by postulating an additional "hedonic tendency" closely akin to McGuire's "wishful thinking." It would be somewhat more difficult, however, to incorporate the less formalized dissonance theory.

If an amalgamation of these theories is to be undertaken, special attention should be given to the "microprocess analysis of cognitive balancing" presented by Rosenberg and Abelson toward the end of their chapter. This new analysis, which borrows some notions from Osgood and Tannenbaum, is only sketched out in broad terms, yet it is highly suggestive. If it could be further formalized and quantified in the manner of McGuire's model, it might well provide a rigorous conceptualization of the many empirical findings resulting from the past few years' research on attitudinal and cognitive consistency. There can be legitimate argument as to when rigorous formalization should be introduced into any area of research, but surely enough is now known about this topic to keep formal theory in touch with empirical reality, and much could be gained from further theoretical integration.

The concentration of this review on basic concepts should not suggest that the book is simply an exposition of theoretical models. It is much more than that. It consists primarily of a reporting of carefully controlled experiments designed to answer specific questions about the nature of attitude change. In one especially interesting study, Rosenberg employs hypnosis to change the affect associated with the object of an attitude. A hypnotized subject is told, for example, "When you awake you will be very much in favor of the idea of Negroes moving into white neighborhoods. The mere idea will make you happy." The subject's cognitive structure is ascertained (i.e., the signs of elements and their relationships) both before and after hypnosis, and it is found that the induced affective change brings about a consistent reorganization of beliefs about the object of affect. Furthermore, when tested a week later, the new beliefs are found to persist. If the hypnotically induced change of affect is then removed, some of the beliefs still persist for at least a short time.

In another series of experiments, Brehm attempts to discover what it is about the occurrence of discrepant behavior that produces dissonance and its associated consequences. This work shows that, even if the discrepant behavior does not actually take place, dissonance may be aroused by the commitment to engage in the behavior. Finally, he presents data to support the conclusion that even commitment to behavior is not required, for simply suffering a disliked fate can arouse dissonance, especially if the subject believes he had a 'choice' that could have prevented his unfortunate condition.

One finding is obtained repeatedly in the several studies: messages are more readily accepted if they are consistent (in the several senses described above) with prevailing affective-cognitive structures than if they are not. It seems, indeed, that there is a preference for consistency.

One measure of the success of a research program is the degree to which it changes the kinds of questions being asked. This book makes such an evaluation easy, for it has an unusually good final section on problems for further research. And tested by this criterion, the research has not done badly. The new questions begin with the assumption that a preference for consistency exists and that the major feature of its effects upon attitude organization and change are known. They probe more deeply now into problems of measurement and quantification, into the details of the processes involved, and into additional variables that may affect these processes. Incidently, anyone seeking a problem for himself or a student can find plenty here, many of them with built-in hypotheses.

Four clusters of problems are emphasized. The first concerns the need for better measurement of degree of inconsistency. The second raises the question of degree of reactivity to inconsistency; what determines the threshold of response to inconsistency? Some pointed questions are posed here about the effects of personality, attitude content, nature of communication, and type of situation. The third deals with the nature of the processes which mediate the changes in attitudinal structures when inconsistency is introduced. The "microprocess analysis" of Rosenberg and Abelson, mentioned earlier, provides a starting point for this line of investigation. The fourth type of problem arises from a certain looseness of conception and method in previous attempts to deal with alternative responses to inconsistency. All the theories considered allow for several, more or less equivalent, consistency-restoring responses to inconsistency, but none of the theories specifies in detail the conditions which bring about one rather than another. Although Rosenberg and Abelson take an important step toward supplying this deficiency, it remains a serious one.

At the very end of the book Hovland and Rosenberg raise a disturbing issue: granted that a preference for consistency has been established, may there not also be a preference for inconsistency? One cannot but wonder, after the issue has been raised, whether the experimental ingenuity of psychologists could have demonstrated so convincingly the existence of this second kind of preference if it had been demanded by their theories. It will not be sufficient to show merely that man is sometimes inconsistent; this would be easy, and present theories allow for it. The question is whether sometimes inconsistency is actively sought out and defended against pressures toward consistency. Surely, somewhere there must be a psychologist sufficiently perverse to undertake this demonstration. His name might even go down in history.

Gestalt Nowadays

Karl Bühler

Das Gestaltprinzip im Leben des Menschen und der Tiere. (Enzyklopädie der Psychologie in Einzeldarstellungen, Band 5.) Bern: Verlag Hans Huber, 1960. Pp. 106.

Reviewed by D. E. Berlyne

Karl Bühler is the Bühler of Külpe's Würzburg School, the man who wrote on the thought processes in 1907, the last papers in that famous series. Bühler followed Külpe to Bonn and Munich and later in 1922 after the First World War succeeded to the chair of psychology at the University of Vienna where he remained until the Nazis discharged him. He had been twice a visiting professor in America, later becoming a consulting psychologist in Los Angeles. He is now at age eighty-two retired. The review tells more about him. The reviewer, Daniel Berlyne, is Associate Professor of Psychology at Boston University. He has an AB from Cambridge University, a PhD from Yale, has taught at the Universities of Aberdeen and St. Andrews and at Brooklyn College, and has worked at Piaget's Centre in Geneva and the behavioral center at Palo Alto. His recent book is Conflict Arousal and Curiosity *(McGraw-Hill, 1960;* CP, *May, 1961,* **6**, *145-147).*

—E. G. B.

The author of this book is one of the great figures of the history of psychology, having established his claim to this status about half a century ago, just before the Gestalt school started to transform the psychological landscape. He is best known for his continuation of the Würzburg school's introspective experiments on thinking. He played an active part in the discussions that marked the advent of the Gestalt school, sympathizing with many of the points they were making while stopping short of their more extreme positions.

And here he is writing many years after the Gestalt school has passed its high-point, at a time when nobody can deny that it left indelible traces behind it but when there can still be a great deal of argument over the exact nature of those traces. It is also a time when we can appreciate the importance of many later developments, without yet being in a position to identify their durable contributions. So we have here an opportunity to learn what an outstanding psychologist, who has lived through many changes of psychological scenery, thinks about some of the key problems of psychology from a contemporary vantage-point. It is a rare opportunity and one that can only be welcomed by anybody with a historical sense.

The present work is the fifth volume of a series entitled *Encyclopedia of Psychology in Monographs,* but the sense in which this series is meant to be more "encyclopedic" than other collections of monographs is not too clear. Bühler, for example, does not aim at a comprehensive exposition of the present status of the Gestalt principle or of its role in the history of psychological theory. Nor does he attempt to provide comprehensive documentation or bibliography. The book consists rather of a succession of passing comments prompted by articles that the author has recently been reading. The topics treated are loosely strung together, and their connection with Gestalt issues is not always fully evident.

Bühler begins with two essays, one by Wellek and one by Herrmann, in which the general conception of Gestalt psychology, including some of the points made in Bühler's own early writings,

are discussed. He then goes on to consider some questions raised by ethology, cybernetics, physiological work on the reticular formation, and writings by Talcott Parsons, Einstein, and Carnap. His overall conclusion is that the ability to react to Gestalten characterizes the higher forms of animal life and separates them from the accomplishments of even the most versatile computing machines.

ENGLISH-SPEAKING psychologists will probably not feel that this book, despite its considerable historical interest, casts much new light on the function of Gestalt concepts in modern psychology. The notion of a Gestalt is not analyzed minutely. The German word *Gestalt* is simply stated to be equivalent to the Latin loan-word *form* and the Greek *morphe*. Gestalt psychology is expounded primarily by contrasting it with earlier associationism and with the kind of reflex theory represented by Pavlov and Watson. The use of Pavlov as a negative model for Gestalt psychology is, however, surely not fully justified here, nor has it been in many previous writings sympathetic to the Gestalt School. After all, Pavlov made his name as a physiologist, long before he studied conditioned reflexes, through his advocacy of "chronic" experiments, on the grounds that one physiological process is invariably affected by what is going on in other parts of the body and interacts with these other processes. Moreover, the best known of Pavlov's several methods of inducing experimental neurosis depends on Gestalt discrimination, that is to say, the discrimination of shape.

The style of discussion is not that to which many English-speaking psychologists, reared on operational definitions and hypothetico-deductive method, are accustomed. It will seem to them reminiscent of an earlier period of psychological debate, although it is a style that has continued to be alive on the European continent. Bühler's arguments are, in any case, here presented too briefly for the merits and possibilities of this kind of discussion to be fairly judged. And most of the points that he makes have been made before.

In general, we must note that Bühler has not incorporated all the important developments with a bearing on Gestalt problems that have appeared since the heyday of the Gestalt school. Anybody's list must be subjective, but many would surely include Bergmann's enumeration of 'good' and 'bad' implications of Gestalt psychology, and the various attempts, from Hull to Hebb, to explain Gestalt perception through principles of associative learning. Although Bühler considers the relevance of computers to psychology and takes up some of von Neumann's ideas, he does not mention recent efforts, with a tangible though so far highly circumscribed degree of success, to program computers for pattern recognition. And Piaget's contentions that perceptual wholes can be fruitfully analyzed in terms of dyadic relations and that the wholes that participate in thinking are radically different from those that participate in perception receive here no mention apart from a bare reference in a footnote.

Clinical Psychometry

Leah Gold Fein

The Three-Dimensional Personality Test: Reliability, Validity and Clinical Implications. New York: International Universities Press, 1961. Pp. xii + 324. $6.75.

Reviewed by AUSTIN GRIGG

Dr. Leah Gold Fein, after taking a Yale PhD in 1944, has been a lecturer at the University of Bridgeport and has had a varied experience in clinical practice in Connecticut. She has served as consultant to Stamford's Alcoholic Clinic, as clinical psychologist with the Norwalk Psychiatric Clinic and with the Roosevelt School in Stamford. The latter two positions she now combines with private practice. Austin Grigg has a history in which broad clinical experience has been combined with teaching and research. He has worked professionally with the feebleminded in Maine, the incarcerated in the Virginia State Penitentiary, with neurotics in the Air Force, psychotics in a mental hospital, the academically troubled in the University of Iowa Counseling Center and with promising clinical students at the University of Texas, where he served for a while as director of the clinical training program. He has recently returned to the University of Richmond, his alma mater.

—F. H. S.

THIS is a mixture of statistical analysis of research data, clinical impressions not submitted to impartial test, extensions of interpretations from one test (Rorschach) to another (3-DPT), and occasional lapses into superficiality even while insisting that the material under consideration "derives from a deep layer" of personality. Not deep, for example, is this judgment: "The 3-DPT is designed to measure personality functions. Rate of production and language variables are functions of personality. Therefore, if the 3-DPT measures these variables consistently, it is a valid instrument for measuring personality functions" (p. 124).

The author has used the test for "over eight years testing about 1,000 subjects" and this book reports her data, her faith, and her diagnostic acumen. Reliabilities, usually reported here as *r*-squared, are unimpressive: the highest is .55 for normals for one scoring category and .90 for 6 catatonics (*r*). There is confusion for the reader about the tables: beginning with Table 22, all N's for various normal and clinical samples have been doubled, but with Table 26, earlier N's are again reported.

Rich clinical examples and the author's interpretations of what various responses may reflect allow the book to be used as a manual for the test. Despite numerous statistical tables, and even a multiple regression, the reviewer judges that the book is intended for the clinician's clinician.

Ease and convenience are poor experimental guides.

—K. M. DALLENBACH

Growth or Regression in Client-centered Therapy?

William U. Snyder, with the collaboration of B. June Snyder

The Psychotherapy Relationship. New York: Macmillan, 1961. Pp. xiv + 418. $7.50.

Reviewed by FREDERICK R. FOSMIRE

The review clearly identifies the author except that it does not say that he is now Professor of Psychology and Director of the Psychological Clinic at the Pennsylvania State University. Frederick R. Fosmire received his PhD at the University of Texas in the early fifties, taught for a while at Montana State then entered the clinical field through a postdoctoral VA internship which led to two years of clinical work in the VA program. He moved to the University of Oregon in 1958 where he is now director of clinical training and where he is involved in research on what he is doing—teaching psychotherapy.

—F. H. S.

DOES therapy work? While outcome research does not give any procedure a gold star for effectiveness, most clinicians reject Eysenck's claim that therapy attempts are futile and instead seem to share Meehl's "faith that therapy 'does something.'" But this simple faith leaves unanswered the questions 'what?' 'when?' 'how?' The shift in research interest from before-and-after studies to investigations into what goes on between theropist and client, with only secondary concern about outcome, resulted partly from difficulties in specifying what constitutes improvement. It also comes from the consensus that it is no longer sufficient to demonstrate that any given procedure works. More important is the question, "What contributes to change within the therapy context?"

Among those who have persisted in a concentrated attack on this question is William U. Snyder, the 'guiding spirit' of the Penn State Psychotherapy Project. The steady flow of research publications from this center is testimony to the vigor with which Snyder has sought to understand psychotherapy. His publications on the topic, spanning most of two decades, indicate the tenure of his interest.

In a review article (Snyder, 1958), Snyder proposed that those who do research on therapy should think of four classes of variables: therapist, patient, procedural factors and interactions between these. Considering the first three domains separately, neither Snyder nor his students seemed to be able to isolate the crucial dimensions, and none of the early work from the Penn State Clinic was as promising as recent studies which suggest that interaction effects are most significant. Anyone familiar with Snyder's writings would expect him to be gratified with this result. Increasingly in recent years Snyder has been of the persuasion that the relationship between therapist and client—their attitudes and feelings about one another—is the consideration *sui generis* in understanding therapy phenomena. *The Psychotherapy Relationship, (TPR),* reports the results of a pilot probe into the effects upon process and outcome of different kinds of relationship formed between the author and 20 clients. Nineteen of the 20 clients were graduate students in psychology, the other was a colleague.

As hypothesis-seeking rather than hypothesis-testing research, the project was successful. The volume yields an abundance of promising leads to future research. To be sure there are methodological problems. Perhaps the most serious of these is the dual relationship between the author (therapist, professor, evaluator) and his clients (patients, students), and the possible influence of $\text{Snyder}_{\text{Experimenter}}$ (S_E) upon $\text{Snyder}_{\text{Instrument}}$ (S_I). How can one assess the extent to which clients' therapy behavior and ratings were influenced by the therapist's potential power over them? When S_I describes "better" and "poorer" clients in terms of Leary's sectors, is he influenced by S_E's obvious sympathy toward the Leary pull hypothesis (that some personality types tend to pull reciprocal reactions)? At a few points, Snyder does not emphasize problems of data as much as this reviewer would have preferred. For example, although he acknowledges Schrier's finding that therapeutic success is positively related to need similarity between therapist and client, he devotes only two sentences to it in spite of the fact that it is contradictory of the pull hypothesis (Schrier, 1953). Even Snyder's results are not as supportive of the pull hypothesis as his favorable attitude toward it might imply. For instance, when he attempted to classify clients into Leary's eight categories on the basis of their EPPS and MMPI profiles, he failed to get even chance concordance. These comments notwithstanding, *TPR* is a valuable contribution to therapy research.

WILLIAM U. SNYDER
B. JUNE SNYDER

WELL worth elaboration into a separate book at a future date is ***TPR's***

bonus feature, a book-within-a-book. To illustrate his approach to therapy, the author presents an illustrative case in some detail, commenting upon his tactics, and occasionally justifying his technique. As Carl Rogers' first PhD at Ohio State and as a frequent writer about client-centered therapy since 1943, Snyder has been closely associated with the Rogerian orientation. To continue to link Snyder with Rogers is to ignore the Rogerian concept of growth, if one may so describe Snyder's drift from Rogerian orthodoxy.

Describing client-centered therapy, Snyder in 1954 wrote: "Among the techniques which the counselor avoids are the giving of information, the giving of advice, the use of reassurance and persuasion, asking questions, offering interpretations, or giving criticisms" (Snyder, 1954, p. 530). In *TPR*, he makes the following comment to justify his request of one client that he discontinue masturbation in hope of enhancing the attractiveness of heterosexuality; " . . . within a framework of learning theory, [the therapist] will attempt to increase all behaviors which will increase drive toward, or increase reward, or decrease punishment of the *desired* behavior, and he will punish all behavior which has the opposite effect" (p. 57). Or, relative to requesting that a client spend time thinking about an interpretation: "This assigning of a task is . . . consistent with learning theory. If the assumption can be made that positive transference exists, i.e., that the relationship is positive, then it is logical to utilize the phenomenon of stimulus generalization in the cause of the therapeutic goal. If the client is favorably disposed toward the therapist, it is hoped that he will be favorably disposed toward suggestions emanating from that source" (p. 60). Also, " . . . the reader will notice the use of two methods by the therapist that occur frequently . . . asking questions, and 'teaching' in the form of showing the client how to make discriminations, or at times, to make appropriate generalizations . . . At times the teaching may take the form of reassurance . . . At times it may constitute an interpretation. The therapist consciously uses teaching methods proudly. Holding to the basic premise that teaching procedures have a long and glorious history of effective production of learning, he accepts the obvious conclusion that there are many situations in which it is appropriate to teach in therapy . . . The argument that this approach is too intellectual, because neurotic behavior is emotional and not cognitive, is cogent at times, but it is comparable to the rationale of the epiphenomenalists, who attempted to distinguish between mind and body. The human adult is rarely wholly emotional or totally cognitive, but exhibits a combination of the two. Almost all behavior above the reflex level has its cognitive elements" (pp. 69-70).

More than a decade ago, Snyder predicted integration among schools of psychotherapy. In what may be a classical case of self-fulfilling prophecy, he has developed an eclecticism that integrates what the reviewer regards as the strongest features of existing therapies. Whether one reads this book principally as a research report or as one experienced and sensitive clinician's views of what constitutes good psychotherapy, he is not likely to be disappointed.

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I do not observe my soul apart from its acts. There are thus processes in the soul of which we are not immediately aware.

—St. Thomas Aquinas (1224-1274)

Perception and the Transactional View

William H. Ittelson

Visual Space Perception. New York: Springer Publishing Company, 1960. Pp. xii + 212. $6.00.

Reviewed by Kenneth N. Ogle

The author, Ittelson, is Professor of Psychology at Brooklyn College where he has been engaged in teaching and research for the last fifteen years. He is well known for his association with Adelbert Ames' contributions to visual space perception and thus with transactional theory. He is the author of The Ames Demonstrations in Perception *(Princeton Univ. Press, 1952). The reviewer, Ogle, for many years associated with the Dartmouth Eye Institute, has been since 1947 with the Section of Biophysics at the Mayo Clinic in Rochester, Minnesota, acting as its Head since 1957. He is the author of* Researches in Binocular Vision *(Saunders, 1950) and* Optics: An introduction for Ophthalmologists *(Thomas, 1961).*

—E. G. B.

Although concerned with perception in its broad sense, this little volume deals primarily with visual space perception from the point of view of the "transaction" theories of John Dewey, A. F. Bentley, and Adelbert Ames, Jr. It is obvious that the author has been influenced greatly by the late Adelbert Ames (to whom the book is dedicated), and in a real sense this book is a more general exposition of Ames' theories and of his visual demonstrations. Such a review is needed. Mr. Ames himself wrote few papers dealing with his own work, and he has tended to rely on others for this commentary. Dr. Ittelson is prominent in this respect, for he wrote the more detailed monograph on the Ames demonstrations (Princeton Press, 1952) after having spent much time with Ames and with the demonstrations.

The author discusses at some length the central problems of perception *(What Is Perception?)*, and this leads him into an exposition of the concept that perception is a *transaction* between the environment and the subjective perceptual experience. The elements of this theory are by no means easy to communicate to the unsophisticated reader. Implied in the theory is the principle of *equivalent configurations* and the assumption of a perceptual *invariance*. The first is the obvious fact of geometrical optics that a given retinal pattern can correspond to an infinite number of different and even unrelated spatial configurations, any two of which, however, can be said to be equivalent. The second (and here is the central problem) attempts to bridge the gap between the retinal stimuli (impingement) and the perceptual experience. It is here that past experience and learning on the part of the observer are called on to assume major roles in providing the choice of those spatial configurations among the possible array of equivalent configurations that corresponds to the world of real objects. In this is involved, then, the "total life situation," so that significances, purposive action, existence of an assumptive world, perceptual weighing, relearning, and so forth—and, later, familiarity and emotional loading—are factors leading to the perceptual experience.

The Ames demonstrations are assumed to constitute evidence for the transactional theory, so the greater part of the book is devoted to a general exposition of the various well-known monocular and binocular cues to spatial localization. Especially considered are those situations in which the cues are made to operate by themselves in a dark room alone, or those situations in which conflicts can be arranged between other controlled cues. The inferences drawn from many of these demonstrations, as having generality, have been criticized (C. C. Pratt, *J. Psychol.*, 1950, **30**, 85-107) on the basis that the viewing situations are so reduced in character that only the role of past experience could be called on to provide the subject with any information whatsoever as to spatial localization.

Great emphasis is placed on the concept of equivalent configurations, not only in monocular vision but also in binocular vision, and it is here that the observations made with the distorted rooms of Ames are considered of such major importance. In monocular vision the principle of equivalent configurations is very clear. In binocular vision, however, from the point of view of this reviewer, a series of strictly equivalent configurations in a three-dimensional world cannot exist (except for very distant objects where both retinal images are identical), because of the vertical disparities between the images in the two eyes for all object points lying out of the horizontal and the median planes of the eyes. The magnitude of these vertical disparities for a given angular position varies with the distance of observation. The conclusions drawn from observations in the distorted rooms considered from the point of view of equivalent configurations need to be examined with care.

Emphasis is also placed by the author on the visual cues of motion parallax and again upon the "physiological cues" from the muscles involved in accommodation and in convergence of the eyes. Care needs to be exercised also in conclusions drawn from these two situations, and especially from the latter, for the Ames demonstrations in this regard cannot be considered conclusive. While there is evidence that some information can be used from gross changes in muscle tonus of the extra-ocular muscles of the eyes, there is a large accumulation of data which tend to show that there is little or no spatial perception from this source. In this regard, it must be said that, if convergence plays a role in spatial localization, then monocular eye-movements also should play a role in the appreciation of size of objects.

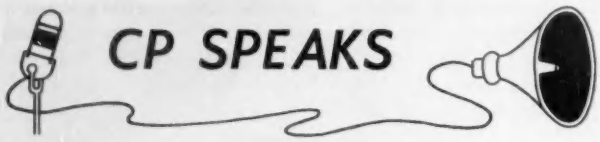
Since the transactional theory implies a kind of weighing of visual factors in terms of the "total life situation," the lapse of time between the impingement of the stimulus on the retina and the emergence of the perception is of considerable, if not of critical, importance. Professor Ittelson is keenly aware of this problem and discusses it in some detail. To this reviewer, however, his citing the time required in the Ames leaf-room for the emergence of a stereoscopic effect does not seem to be pertinent to the basic problem. Variations between subjects are tremendous in this respect; the times are comparatively large, and too many factors are involved to make the examples convincing.

Since Dr. Ittelson has not summarized his statements at the close of the book, it is rather difficult for the reader to assess the demonstrations as evidence for the transactional theory in contrast to other possible theories. In concluding he seems to expand his notions of perception somewhat so as to include emotional factors in the perception of persons and even in the perception of personal relationships, and to some extent in the perception of objects. This extension of his view has led him directly into problems of interest to psychiatry in considering the dominant perception of unlike and emotionally loaded figures observed in binocular rivalry in the haploscope. This discussion here leaves the reviewer, who admittedly is biased in his efforts to understand visual perception in terms as close as possible to those of psychophysics and physiologic mechanisms, with mixed feelings.

In spite of these different points of view, this little volume is indeed a contribution to the effort to understand visual space perception. The readable review of some of Ames' demonstrations alone makes the book an asset to one's library on perception.

For many parts of nature can neither be invented with sufficient subtility nor demonstrated with sufficient perspicuity nor accommodated unto use with sufficient subtility nor demonstrated with sufficient dexterity, without the aid and intervening of mathematics.

—Francis Bacon



? ? ?

For three days and three nights the editor of *CP*, functioning as a scribe, wrote and revised and reworded and rewrote what was intended as a fine and erudite screed for this space. It dealt with some basic confusions about what is a book, what is psychology and who is a psychologist. The piece was set aside for a day while the editor, in the role of house-painter, made a significant difference in some ceilings. Then the editor, as editor, evaluated the piece; he sent it back to its author with the decision that it would be better to print the ceilings.

Emergency. At the printer's establishment fifty-six typesetters were holding their breath waiting for a manuscript they could cast into print; there was no printable manuscript. In such a situation there are a number of possible responses. Cut and run. Leave the space blank. Or print a cheerful "good morning" in the middle of a vast expanse of white. No. *CP's* editor ought to say something. The journal belongs to psychologists and its steward ought to stay in active and if possible expressive contact with those for whom he works. He should do what he can to reveal his biases. He should state his principles, if any, and should make explicit his own interpretations of the rules under which he thinks he is operating. But he still should not try to declare himself when his declarations reveal little more than intellectual nudity or attitudinal wishywashiness. Let him then—and here is the simple solution—ask questions. The major trouble with the first piece was that it set down declarative sentences about matters on which its author, either through ignorance or because of attitudinal dissonance, was not yet ready to declare anything much. So let's beat a strategic retreat to questions. Questions are easier. And they can be highly recommended on other grounds. Questions do communicate, and do reveal something of the one who phrases them. More significantly, questions can elicit responses that not only can prevent the too early formulation of assertions but also can contribute to the quality of that enlightened confusion upon which complex decisions eventually can be made.

So. Questions.

What, when you get down to it, is a book? How does a book differ from a monograph? Under what circumstances can a volume lose its hard shell and still remain a book? For *CP's* purposes, is a reprint of a book still a book? Is a revision a book, a reviewable book?

What is the difference between a compendium and a book? Or under what circumstances is a compendium a book? Is a published symposium truly a book? How does a compendium or a symposium differ from a bound volume of the *Journal of Experimental Psychology*? Why do *CP's* reviewers have such difficulty with symposia? And what does *CP* do about such volumes as the *Annual Review of Psychology* or about the recent volume on *Sensory Communication* in which, under the editorship of Walter Rosenblith, there are forty separate contributions from 44 separate individuals?

Let us assume that it can be decided, by brute force or otherwise, whether a given object received from a publisher is indeed a book. How do we then decide on its appropriateness for *CP*? The Journal seems generally to be committed to reviews, done most often by psychologists and for psychologists, of psychological books. But when is a book psychological? And, from *CP's* bookish point of view, what is psychology? Can psychology—and the substantive concern of *CP*—be defined as what psychologists do? Suppose a proper psychologist (and if one lets in even a bit of niggling uncertainty, that's a troublesome term too) writes a book on skiing or plumbing, or on a system for betting on horse races or on the art and science of reddogging? Does *CP* review it? Is psychology *everything* psychologists do?

Would we do any better to define as psychological those things psychologists are interested in? Or would we then still face the necessity of differentiating between psychologists and people? When are psychologists psychologists and when are they people? Many psychologists are interested in music. Does *CP* then review books on music? Or on the hierarchiality of social structure in universities? Or on income tax laws?

Take the matter of sex. Psychologists write about sex. Psychologists are interested in sex. *CP* reviews books—some books—on sex. But sex continues to give *CP* trouble. Perhaps it can be educative trouble.

Sitting now on *CP's* shelves, suspended in uncertainty, are the following books on sex:

Gottlieb, Berhardt S. *What a Girl Should Know About Sex*. Indianapolis: Bobbs-Merrill, 1961. Pp. 190. $3.25.

Wedeck, Harry E. *Dictionary of Aphrodisiacs*. New York: Philosophical Library, Inc., 1961. Pp. 256. $10.00.

Kyle-Keith, Richard. *The High Price of Pornography*. Washington, D. C.: Public Affairs Press, 1961. Pp. vii + 230. $4.50.

Caprio, Frank S. & Donald R. Brenner. *Sexual Behavior: Psycho-Legal Aspects*. New York: Citadel Press, 1961. Pp. 384. $6.00.

Hegeler, Sten. *Peter and Caroline: A Child Asks About Childbirth and Sex*. (trans. from the Dutch) New York: Abelard-Schuman, 1961. (first published in English in 1957 by Tavistock, London.) Pp. 34. $2.00.

Driver, Helen I. (Ed.) *Sex Guidance for Your Child: A Parent Handbook*. Madison, Wis.: Monona Publications, 1960. Pp. 192. $4.50.

Bergler, Edmund. *Counterfeit-Sex: Homosexuality, Impotence, Frigidity*. New York: Grove Press, Inc., 1961. Pp. vii + 380. $2.95.

Should *CP* review any of these books? All of them? *CP* does review books on sex. During the past two years it has

carried reviews of the following books:

Eugene de Savitsch. *Homosexuality. Transvestism and Change of Sex.* (Charles C Thomas, 1958; *CP,* Feb. 1960, **2,** 48-50.) Reviewed by Daniel G. Brown.

Dorothy Walter Baruch. *New Ways in Sex Education: A Guide for Parents and Teachers.* (McGraw-Hill, 1959; *CP,* Mar. 1960, **3,** 99-100.) Reviewed by Ralph H. Ojemann.

Richard C. Robertiello. *Voyage from Lesbos: The Psychoanalysis of a Female Homosexual.* (Citadel Press, 1959; *CP,* Apr. 1960, **4,** 123.) Reviewed by Evelyn Hooker.

Edrita Fried. *The Ego in Love and Sexuality.* (Grune & Stratton, 1960; *CP,* Feb. 1961, **2,** 38-39.) Reviewed by Albert Ellis.

Phyllis Kronhausen and Eberhard Kronhausen. *Sex Histories of American College Men.* (Ballantine Books, 1960; *CP,* July 1961, **7,** 249-250.) Reviewed by Lester A. Kirkendall.

J. Donner. *Women in Trouble.* (Monarch, 1959; *CP,* Oct. 1960, **10,** 333.) Reviewed by Edward J. Murray.

E. Kronhausen, and P. Kronhausen. *Pornography and the Law.* (Ballantine, 1959; *CP,* Oct. 1960, **10,** 333.) Reviewed by Edward J. Murray.

A. Ellis. *Sex Without Guilt.* (Hillman, 1959; *CP,* Oct. 1960, **10,** 333.) Reviewed by Edward J. Murray.

B. Russell. *Marriage and Morals.* (Bantam, 1959; *CP,* Oct. 1960, **10,** 333-334.) Reviewed by Edward J. Murray.

Do the two lists differ? In what ways? When is sex psychological? When is sex scholarly or scientific rather than artistic or practical or plainly human? And are the foregoing proper questions for *CP* to ask about sex?

Not only do books on sex fall frequently on or near *CP's* indistinctly drawn line. What about books written by psychologists for non-psychologists? And what do we do with books written by non-psychologists for non-psychologists but on psychological matters? What should *CP* do about such books as the following, all now at hand:

Brussel, James A. *The Layman's Guide to Psychiatry.* New York: Barnes & Noble, Inc., 1961. Pp. vii + 235. $1.50.

Laird, Donald A. & Eleanor C. Laird. *Tired Feelings and How to Master Them: A Practical Summary of Techniques for Home and Business.* New York: McGraw-Hill, 1961. Pp. x + 198. $4.50.

Coue, Emile & C. H. Brooks. *Better and Better Every Day: Two Classic Texts on the Healing Power of the Mind.* Part I: *Self Mastery Through Conscious Autosuggestion,* by Emile Coue. Part II: *The Practice of Autosuggestion by the Method of Emile Coue,* by C. H. Brooks. New York: Barnes & Noble, 1961. Pp. 158. $.95.

Adams, Anderson. *How to Use Self-Hypnosis for Health and Wealth.* New York: Exposition Press, 1961. Pp. 40. $2.50.

Wood, James Playsted. *Advertising and the Soul's Belly: Repetition and Memory in Advertising.* Athens: University of Georgia Press, 1961. Pp. xii + 116. $3.50.

CP, through its established procedures will continue making decisions about each of the books on sex it receives and about each of the books in whatever the category that fits those in the last list above. Is there a better way to make decisions than to consider many angles, to weigh assorted input from varied consultants and then to decide on non-principled impulse? Is there no comfort to be had in a non-Aristotelian world?

—F. H. S.

A Hotbed of Neurosis?

Albert Ellis and **Robert A. Harper**

Creative Marriage. New York: Lyle Stuart, 1961. Pp. 288. $5.00

Reviewed by Clarence Leuba

Both Albert Ellis and Robert Harper are experienced psychotherapists and marriage counselors, both are prolific writers. Among Ellis' recent books are Sex without Guilt *(1958)* and What is Psychotherapy *(1959). Harper's books include* Marriage *(1940) and* Psychoanalysis and Psychotherapy: 36 Systems *(1959). Clarence Leuba, long-time chairman of the department at Antioch College, is a general psychologist, evidence of which classification is furnished by his new introductory text:* Man: a General Psychology *(1961). His breadth, however, has not prevented the development of special interests, one of which has been a persisting concern, both academic and applied, with marriage. He has regularly taught courses in marriage and has engaged in marital counseling with students and others. He also has written a book on* Ethics and Sex Conduct *(1948) and a smaller one on* The Sexual Nature of Man *(1955).*

—F. H. S.

Both Ellis and Harper have had long experience as marriage counselors and psychotherapists. This experience, plus familiarity with the pertinent scientific literature and a flair for clear, vigorous writing, has resulted in a book valuable for those beset by marital problems—and also for those concerned with improving their already fairly satisfactory marital relations. Emphasis is on the compatibility, love, and sexual aspects of marriage, and on viewing these rationally and realistically. Throughout the book there is a good balance between generalizations and their clarification by specific cases from the authors' files.

As marriage counselors and psychotherapists, the authors see mainly the gloom in marriage; even to the extent of entitling their first chapter: "Modern Marriage—Hotbed of Neurosis." They envisage American culture, especially in its sexually repressive aspects, as the main villian in marital disturbances.

Their own view of sex is an unsentimental, unglamorized, and realistic one. They convey this view straight from the shoulder and they expect their readers and clients to accept it without adding any romantic nonsense. They think sex should be fun, like athletics, and those who cannot take this frank, matter-of-fact view of sex had better seek professional help.

Ellis and Harper rely heavily on a rational approach to marital problems. After repeated, forceful, and rational explanation of his difficulties, a client is supposed to accept this explanation and to act accordingly. The book is rich in case studies in which this actually happens. Clients see the light and

gratefully follow the plans made for them.

The authors have been much impressed by the dire results of the overwhelming shame and guilt produced in their clients by what the authors consider as natural and inevitable violations of conventional codes of sexual behavior; they are led accordingly to condone such violations. Though they seem to favor fidelity in marriage, as contributing to stable and satisfying marriages, they claim that practically everyone will inevitably have extramarital desires and fantasies of a sexual sort. Married people should not feel bad about such imaginings; illicit desires should merely be kept in the fantasy realm. Actualizing them need not, however, lead to blame or undue emotional upheavels. The situation should be examined rationally and dealt with accordingly.

ALTHOUGH it may be that excessive shame and guilt devastate the marital lives of the large number of neurotic individuals seen by the authors, one may hesitate to follow the authors in their apparent belief in the significance of the shame—guilt factor in marriage and sex in general. Granted that sex should be, and can be, fun, is that the main thing to emphasize when dealing with non-neurotic people?

Your reviewer is reminded of a cartoon in which a nursery school scene is depicted: one little boy has just knocked out a playmate by hitting the latter over the head with a mallet. A nursery school teacher holds the assailant in her arms while the victim lies unattended on the floor, and remarks: "The important thing is that we not make him feel guilty." The author's neurotic clients may feel too guilty; but as Mowrer and others have pointed out, it could be that most people do not feel guilty enough. In any case, how is fidelity in marriage to be achieved unless at least the beginnings of sexual control have been attained premaritally? Can one count automatically on marriage to produce fidelity?

Ellis and Harper wisely insist that marriage is not for everyone; and they indicate some kinds of people who are probably better off unmarried. They also describe what some people might do to make marriage more attractive and suitable for themselves.

In dealing with marital troubles the authors discuss in a helpful, practical way such topics as the improvement of communication between husband and wife and incompatibilities based on fundamental diversities of values and interests. They contrast such incompatibilities—which may well necessitate divorce—with those based on possibly changeable neurotic attitudes.

All in all, the authors are at their best when, out of their rich experience, they describe sources of difficulty in marriage and suggest solutions. The directive method they use in marriage counseling and in psychotherapy seem to work well for them. Most open to question, it seems to your reviewer, are the extensions of their ideas about sex and love to normal everyday marriages. It is doubtful that such marriages are "hotbeds of neurosis."

Personality Perplexities

Ira Iscoe and Harold W. Stevenson (Eds.)

Personality Development in Children. Austin: University of Texas Press, 1960. Pp. viii + 171. $4.00.

Reviewed by HELEN L. KOCH

Editor Iscoe is Associate Professor of Psychology at the University of Texas, a member of the State Board of Hospitals and Schools and of the staff of the Austin Cerebral Palsy Center, and a consultant for the Hogg Foundation for Mental Health. His colleague, Stevenson, who was at Texas until 1959, is now Professor of Psychology at the University of Minnesota and Director of the Institute for Child Development and Welfare there. The reviewer, Helen Koch, is now Professor of Child Psychology Emeritus at the University of Chicago. She has long been chiefly concerned with the social development of the child and most recently has been writing on the effects that siblings of various types have on each other's social attitudes.

—E. G. B.

THE volume is a collection of papers read during a lecture series occasioned by the University of Texas taking note of the seventy-fifth anniversary of its founding. As in most such collections, the content is a rehash (with the exception of possibly one paper) of material presented more adequately and fully elsewhere. Also, as usual, the papers are of uneven merit, allowing for no development of a system of ideas nor detailed analysis of an array of data. In fact, it is only because personality is such a global, catch-all concept that the various presentations can be said to fall within the same field.

The papers are, however, thoughtful products of competent men. The psychologists are J. E. Anderson, R. R. Sears, and the late H. E. Jones; the psychiatrist is M. J. E. Senn; the anthropologist is J. M. W. Whiting; the sociologist is O. G. Brim. They are concerned largely with socially induced and socially significant behavior.

The topics covered range from the stability of a given trait (e.g., reserve vs. overt expressiveness) or achievement (e.g., social adjustment) through time, from factors which influence such mental structures as conscience or identification, up to a theory of role functioning offered to account for what are believed to be improperly emphasized behavioral consistencies. The latter theory is tendered by its author, O. G. Brim, as more useful than the notion that major behavioral consistencies—and there are apparently few of them—are largely situationally or genetically determined. The platitudinous theme is belabored by one author that, since behavior is complex, a multi-disciplinary approach to its study is desirable and that the medical disciplines should have a strategic role to play in any joint effort.

The reviewer has pondered the question of the value of the collection and sees it as serving as a sort of eye-opener for the intelligent layman or for the young student of personality. The opus gives a view of some of the problems

faced by the scientist who tries to formulate useful concepts in terms of which to plot personal-social development. It raises questions concerning some current dogmas (e.g., that there is a dynamic relation between breast feeding and later social behavior). It points up the astounding dearth of *bona fide* developmental studies. It introduces some new instruments. Also it offers a description of the attack made by some social scientists on a few relevant problems.

WHITING presents a more than usually simple, non-mystical view of identification as merely the wish, plus the attendant effort, to become like another in order to acquire control of resources (money, power, skills, status) like the other. Resources will have been withheld from the aspirant, at least at times. Love plays less of a role in Whiting's concept than in most.

Brim defends the thesis that social man's behavior can often be well understood in terms of what he has learned about the expectations those around him hold for individuals in given roles as well as about some of the means for meeting these expectations. The theory is proposed to account for apparent inconsistencies in behavior (e.g., between the meek husband and tyrannical boss) as well as for consistencies (e.g., a father's persistent indulgence and protection of a wayward and rebellious son). Since roles are admitted to be infinite in number and learning of the details of roles is never ending, one may, of course, with this theory, end in a bog not much less overwhelming than that in which the trait theorist is so often mired. Whether Brim's plotting of the developmental picture takes adequately into account the effect of constancies in the physical environment or in the biological sphere is a question. His view, however, seems to this reviewer to have promise; but, as he himself suggests, it needs a searching kind of testing.

The Jones paper is a scholarly one. Among its several achievements it leaves one optimistic about the social health of the reserved person, as opposed to the overtly expressive. Since the extrovert has long been the approved model, it is satisfying to see the introvert come into his own. Also the distinction that Jones draws between the formally and informally sociable is attractive. Moreover, his discovery of some of the antecedents and consequents of the two types of social functioning in adolescents helps to illuminate a problem that has been neglected.

The Self in Chaos

Ruth C. Wylie

The Self Concept: A Critical Survey of Pertinent Research Literature. Lincoln: University of Nebraska Press, 1961. Pp. xiv + 370. $4.50.

Reviewed by A. W. COMBS

Dr. Ruth Wylie, a Pittsburgh PhD, has carried out her teaching and research at Stephens College, at Connecticut College for Women and, more recently, at Sarah Lawrence College. Her research, forwarded both in academic settings and during periodic ventures off the campus, has been consistently concerned with the use of experimental and other objective methods of testing hypotheses derived from personality theory. In 1961-62 she holds a National Science Foundation Faculty Fellowship. Arthur W. Combs, the reviewer, received his PhD in 1945 from Ohio State, where he worked with Carl Rogers and where he was initiated into research on and applications of client-centered therapy. During more than ten years at Syracuse University. Dr. Combs published prolifically while he was developing the training program in clinical psychology and directing the University Personal Counseling Service. Since 1954 he has been at the University of Florida, where he is Professor of Education and Psychology in the School of education. His writing, though varied, has dealt most extensively with aspects of client-centered psychotherapy. He co-authored, with Snyder et al., Casebook of Non-Directive Therapy *and, with Syngg,* Individual Psychology: A New Frame of Reference for Psychology. *His present research interest lies in a longitudinal study of childrens' perceptions of themselves, a long term study supported by the United States Office of Education.*

—F. H. S.

FOR some time we have needed a review of the research on the self concept that would help to bring order out of the avalanche of studies pouring from the perceptual, phenomenological, self psychologists among us. One, therefore, approaches a book with the title, *The Self Concept,* with relish and anticipation. But, alas, he is doomed to disappointment. Four hundred ninety three publications on the self concept and its related aspects are critically reviewed in this book. Yet, the reader emerges at the final pages more bewildered and confused than he began, and this, through no fault of the author! Dr. Wylie has labored mightily to bring some order out of the current confusion in regard to the self. Her approach is one of analyzing this vast new field in methodological and empirical terms. Unfortunately, the sickness goes much deeper to basic problems of conceptualization.

It is doubtful if our science has ever witnessed a more chaotic state of affairs with respect to one of its concepts. Our literature is awash with studies, ostensibly on the self concept, but which turn out, on closer analysis, to be studies of the self report. Only a very few of the studies reviewed in this volume can properly be described as researches on the self concept despite the fact that they are labeled so! Self theorists have defined the self concept as what an individual *believes* he is. The self report, on the other hand, is what the subject is ready, willing, able or can be tricked to *say* he is. Clearly, these concepts are by no means the same. Yet, amazingly,

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experiment after experiment reported in this book is designed as though they were. Dr. Wylie, herself, has fallen into this trap. Though she describes the self concept in the opening paragraph of her book as having to do with the "self as the individual is known to himself," throughout the remainder of the text she appears to accept without protest self report studies as bearing directly upon the self concept.

Psychology turned its back upon introspection as a valid technique for personality study two generations ago. Most of the studies reported here, however, are dependent upon a naive acceptance of the subject's report about himself as a valid measure of the self concept. The experiments described in the volume show far more subtlety than those of fifty years ago to be sure. But the use of *Q* sort technique and fancy statistical manipulations should not obscure the fact that asking a person how things are with him is plain and simple introspection. What an incredible state of affairs! Even if the procedural sophistication and experimental rigor Dr. Wylie so eloquently calls for were to be had, what does it mean if "statistically significant" findings are obtained for the wrong problem? How shall we handle research reports mislabeled throughout the literature?

THE *Self Concept* begins with a general discussion of research design with particular attention to a comparison of *S-R* and *R-R* approaches to measurement. This is followed by a long chapter on measurement in which such devices as *Q* sorts, questionnaires, rating scales, check lists and the like are critically examined. Several tables listing instruments in common use will be particularly helpful to persons considering research in this area. The remainder of the text is devoted to critical review of a large number of studies using, more or less successfully, these approaches to measurement. A short final chapter draws a number of conclusions and implications. The author makes no attempt to criticize studies from a conceptual view. Instead, she has attempted to analyze the field in veridical terms from the traditional frame of reference of objective, normative psychology, and does it very well. She has a fine grasp of research technique and psychologists will generally find her criticisms penetrating and provocative.

It will no doubt come as a surprise to other phenomenologists reading this book, as it did to me, to find that we are called by this name because of our "stress on the role of the conscious self concept in determining a person's behavior." The author apparently shares the common misconception that phenomenology deals only with conscious aspects of perception. The perceptual view as I have always understood it holds, however, that behavior is a function of the phenomenal field which includes *all levels* of the person's awareness. Indeed, one of the contributions of this point of view is its elimination of the unfortunate distinctions implied by the terms "conscious" and "unconscious." Certainly Dr. Wylie has the privilege of defining her terms as she pleases, but the separation of the self into phenomenal (conscious) self and nonphenomenal (unconscious) self seems particularly misleading and can only add to the confusion already existing over definitions of self.

In the concluding chapter the author says, "In short, the total accumulation of substantive findings is disappointing, especially in proportion to the great amount of effort which has been expended." This reviewer can only add a heartfelt "Amen!" Perhaps the first step toward clarifying our thinking and tightening up our conceptualization and research design, however, is just such a compilation of studies as Dr. Wylie has here provided us. She has done us a real service in providing this critical review. Although one might wish it had been more accurately titled *The Self Report,* it is, nevertheless, a source book many a research designer will want to have close at hand. In time, it is to be hoped, one of the perceptual, phenomenological psychologists among us will write another volume dealing with the problems in conceptual terms as well.

Truth, unlike the Big Lie, does not increase by repetition.

—ALBERT EINSTEIN

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The Mental Healths of Communities

Charles C. Hughes, Marc-Adelard Tremblay, Robert N. Rapoport, and Alexander H. Leighton

People of Cove and Woodlot: Communities from the Viewpoint of Social Psychiatry. (Stirling County Study of Psychiatric Disorder and Sociocultural Environment, Vol. II.) New York: Basic Books, 1960. Pp. x + 574. $10.00.

Reviewed by E. GARTLY JACO

The present volume is the second of the series directed by Alexander Leighton, who is Professor of Psychiatry and Sociology at Cornell University and who published the first volume, My Name Is Legion *(CP, Dec. 1960,* **5,** *395-398). Charles Hughes is his Associate Director in this Stirling County Study and a Senior Research Associate in Cornell's Department of Sociology and Anthropology. Tremblay is Associate Professor of Sociology in Laval University in Quebec, and Rapoport is Lecturer in Social Anthropology at Harvard University. The reviewer, Jaco, one of the first sociologists to enter field of medicine as a sociologist, is associated in various professional ways with the Department of Sociology, the Department of Psychiatry and the School of Applied Social Sciences at Western Reserve University. He is the editor of* Patients, Physicians and Illness *(Free Press, 1959; CP, Mar. 1960,* **5,** *96-98) and the author of* The Social Epidemiology of Mental Disorder *(Russell Sage Foundation, 1960). He is co-editor of the recently founded* Journal of Health and Human Behavior. *He used to be Director of the Laboratory of Socio-Environmental Studies at the Cleveland Psychiatric Institute and director of the Division of Medical Sociology at the Medical Branch of the University of Texas.*

—E. G. B.

SOCIAL psychiatry means different things to different people. In the search for so-called 'social factors' in psychiatric illness, numerous disciplines with varied orientations and approaches often cross paths. These cross-cultural contacts can lead to positive, negative, or confusing developments. The sociologist, the anthropologist, the psychologist, the psychiatrist, and the epidemiologist may pursue their own dependent variables in terms of their different frames of reference and methodologies, or they may attempt to pool their contribution into a collaborative effort. Some would argue that the specific psychiatric case is the universal dependent variable for all disciplines involved in social psychiatry; others would hold that each discipline should retain its own identity and specific contribution to the total explanation of mental illness. Few would argue today that only one of these disciplines has a monopoly on social psychiatry. As a consequence, the bringing together under one roof of representatives of various behavioral sciences and health disciplines in a team effort is becoming commonplace in contemporary research in social psychiatry. Such an attempt is reported in the series of volumes now being published on the *Stirling County Study of Psychiatric Disorder and Sociocultural Environment,* conducted by Dr. Alexander H. Leighton and his associates.

People of Cove and Woodlot is the second volume in this series, following *My Name is Legion* which appeared in 1959 *(CP,* Dec. 1960, **5,** 395-398). An appreciation of the second book is partially contingent upon a thorough familiarity with the first, since the first volume presented the general orientation, theoretical premises, and the overall research program from which the present book is derived. The second volume is concerned with part of this ambitious endeavor, as stated by the authors:

> Choose a population in the Northeast of America such that there are distinctive geographic boundaries and marked contrasts of community types within these boundaries. The contrasts should include the presence of both French and English cultural groups.
>
> Determine the main demographic, economic, sociocultural, and historical features.
>
> Select a number of communities that are markedly different in degree of social disintegration, and see that French and English cultures are represented at each extreme. Let a plurality of the following items be used as preliminary indicators (Set B) of disintegration:
>
> 1. A recent history of disaster
> 2. Widespread ill health
> 3. Extensive poverty
> 4. Cultural confusion
> 5. Widespread secularization
> 6. Extensive migration
> 7. Rapid and widespread social change
>
> The communities selected through these indicators are to be further assessed by means of the following confirmatory indicators (Set A) of disintegration:
>
> 1. High frequency of broken homes
> 2. Few and weak associations
> 3. Few and weak leaders
> 4. Few patterns of recreation
> 5. High frequency of hostility
> 6. High frequency of crime and delinquency
> 7. Weak and fragmented network of communications
>
> Provide a descriptive account of these communities which will depict their functioning as self-integrating units. This should include, as a focal point, a statement of the sentiments most salient with regard to this functioning.

Of the seven "preliminary indicators" (Set B) of social disintegration, only three were used in selecting the specific communities: poverty, "cultural confusion" (intercultural mixing or ethnic heterogeneity), and secularization. Two communities categorized as reasonably well-integrated were delineated within the study-county, and four areas as disintegrated. The bulk of this second volume is a description of these selected communities in terms of their demographic characteristics; patterns of subsistence, community, religious, and family life (which includes infancy and childhood experience), and sentiments (defined as "a union, or intersection, of

cognitive and affective processes which have duration along the life-arc"). These latter factors embraced the "Set A indicators" of degree of disintegration as they varied between the different communities studied.

Leighton summarizes this volume in capsule form: "the ground has [now] been prepared for the epidemiological findings to be given in Volume III; a sociocultural map has been laid out upon which can be projected the frequencies of symptoms of psychiatric significance."

The final chapter, "Sociocultural Process and Psychiatric Disorder," attempts to recapitulate the general findings and place them in perspective with the entire project. In a sense, this final chapter is a prologomenon to the next volume. Sociocultural processes conducive to integration and disintegration with some discussion of *anomie* and other concepts already existing in the social psychiatric literature are presented, followed by a discussion of psychiatric theory of the functioning of personality as related to these sociocultural processes within the study-communities.

THE Stirling County study is a notable and noble effort in social psychiatry. Anyone reading both of the volumes in this series cannot help but be impressed by the project's magnitude, the exhaustive field reports, the volumes' intensive and extensive conceptualizations and hypothesis-building, and the patient and controlled teamwork of the representatives of the many disciplines involved. The psychologist, the social scientist, the psychiatrist, the public health specialist, and the laymen can find something of interest and value in both volumes.

Yet many critical questions come to mind as one reviews this formidable work. The intermixing of levels of concepts, frames of reference, and empirical data from the different disciplines makes interpretation and evaluation of the various sections of the project difficult and confusing. This reviewer found it a constant chore to keep in focus the descriptions of the study-communities and their pertinence to the stated purposes of the psychiatric components of the total project.

A question can be raised about the basic research design. Why begin the project with selected communities in terms of factors held to be of social psychiatric significance and then seek psychiatric cases in these areas for comparative analysis, rather than the reverse, by determining areas with differing psychiatric censuses and then delineating any sociocultural differentials that may be related to psychiatric variables? The former procedure is logically somewhat circular, since it accepts at the outset certain social psychiatric premises which it promises subsequently to test in the field. That is to say, the so-called "indicators" of social disintegration, labeled as Sets A and B in this project (poverty, ethnic hetereogeneity, secularization, broken homes, social isolation, hostility, and other indices of social disorganization, etc.), have already been found by numerous studies to be associated with high rates of mental disorders. Such studies, indeed, already comprise the bulk of the prevailing literature in social psychiatry and offer justification for using the very rubric termed "social psychiatry." There are thus no surprises when it is found that the "disintegrated communities" have a higher frequency of psychiatric symptoms, or that some social areas exhibited more psychoneuroses and psychophysiological patterns, while still others had more personality and sociopathic disorders.

An even more basic methodological problem is the use of prevalence as opposed to incidence rates in this project. Perhaps it is an unjust criticism of this study, due to its inception more than a decade ago, but it is regarded by many today as no longer tenable to relate prevalence rates, true or otherwise, to etiologic principles or variables in mental illness. Rather, the incidence rate is more appropriate to this type of research, to the limited extent, of course, that epidemiology can contribute to etiology. Kramer was one of the first to point this out when he wrote: "If we are to learn more about the role played by socioenvironmental variables in the production of mental disorder, then we must extend our knowledge of the incidence of these disorders in various population groups. Similarly, if we are to understand the influence of these variables on the course of specific mental disorders, then we must also study variations in remission, relapse, and mortality rates. Studies of prevalence alone leaves these basic questions unanswered" (Morton Kramer, "A discussion of the concepts of incidence and prevalence as related to epidemiologic studies of mental disorders," *American Journal of Public Health*, 1957, **47**, 826-840). Factors related to the onset of *new* psychiatric cases, rather than to the prevailing cases, in a study-population, therefore have more etiologic significance. Consequently, the association of prevalence rates of mental illness to sociocultural conditions in these communities or even the lack of them will not properly test the hypotheses of this inquiry, since factors related to prevalence may not *necessarily* be connected to incidence, (the production of new cases) nor to etiology.

It was also disturbing to this reviewer to find 'serendipity' playing such a frequent role throughout this project. The authors frequently "discovered" the potential importance of certain already known social and cultural factors related to mental illness, thus indicating a failure to recognize or acknowledge much sociological and social-psychological research and theory now prevailing in social psychiatry. Indeed, they admit this point (p. 432): "The fact that our concern with noxious influences on mental health has led us out into this familiar territory (sociocultural processes) is perhaps worthy of note. It was certainly not in our thinking at the beginning."

These criticisms are, however, of only a few specific components of the total endeavor. Leighton and his associates are to be congratulated for their tremendous effort that will contribute much to social psychiatry. Their frank reporting in detail of the many facets and developments of the project will be likely to be as much of a contribution as their theoretical formulations and empirical findings. The final judgment of this enormous enterprise must, however, be suspended until the complete series of volumes has appeared and the work then evaluated *in toto* rather than by any of its parts.

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Personality and Medical Education

Kenneth R. Hammond and Fred Kern, Jr., with Wayman J. Crow, John H. Githens, Byron Groesbeck, John W. Gyr, and Lyle H. Saunders.

Teaching Comprehensive Medical Care: A Psychological Study of a Change in Medical Education. Cambridge, Mass.: Harvard University Press, for the Commonwealth Fund, 1959. Pp. xxii + 642. $10.00.

Reviewed by JOSEPH D. MATARAZZO

Of the seven authors of the present book, two are listed in large type on the dust cover. The first of these, Kenneth R. Hammond is a psychologist who learned and taught at the University of California at Berkeley and who is now Director of Research at the Behavior Research Laboratory of the University of Colorado. Fred Kern, Jr., the second large-type author is a physician who directs the General Medical Clinic at the University of Colorado Medical School. Of the other authors, Wayman Crow, Byron Groesbeck, and John Gyr are psychologists and research associates of the Behavior Research Laboratory. John Githens, a physician, is assistant director of the General Medical Clinic and Lyle Saunders is an associate professor in the Colorado School of Medicine. Joseph Matarazzo, since his Northwestern PhD in 1952, has had continued and intimate exposure to medical psychology and to medical education, first at the Washington University School of Medicine, then at the Massachusetts General Hospital and the Harvard Medical School and, most recently, at the University of Oregon Medical School where he is chairman of the very new Department of Medical Psychology. From 1950 to 1955 he was involved in a clinic that was similar to that reported here but located at Washington University.

—F. H. S.

EDUCATION as a social institution has been practiced by mankind for many thousands of years. Yet, interestingly despite numerous *isolated* studies on teaching methods, on the one hand, and the process of learning and the individual attributes of the learner, on the other, few studies have ever been reported of large scale, systematic investigations of the learner, teacher, their respective as well as common environments and the interactions of one of these persons with the other. This book is a report of such an investigation.

The study, one of several generously supported by the Commonwealth Fund during the period following the Second World War, had its inception in the discontent found among leading medical educators regarding the over-emphasis on medical specialization brought about by the many increases in medical-scientific knowledge during the last fifty years. Specifically, some of these medical educators came to believe that the central objective of medicine, the coordinated care of the individual patient, including provisions for the role of socio-psychological factors in his disorder, had given way to an impersonal interest in him as a conglomeration of specialized organs and bodily systems—each to be treated by a different physician-specialist.

It was in an attempt to reverse this trend in medical education that the administration and faculty of the University of Colorado School of Medicine in 1953 initiated a newly organized outpatient faculty, called the General Medical Clinic (GMC), for the purpose of teaching and demonstrating the principles of comprehensive medical care to senior medical students. The senior class for three consecutive years (1954, 1955 and 1956) was therefore randomly divided, for research purposes, with one half receiving its medical outpatient education in the regular curriculum (the specialty clinics) and the other half receiving its education in the newly introduced General Medical Clinic (with its emphasis on coordinated, comprehensive care of the whole patient). The book describes both the research methods employed in studying these two teaching approaches and the results of this educational experiment.

To assess the expected differences between the curricula for the control and experimental groups ("why the difference happened") the following techniques were utilized by the Independent Variables Section of the Research Team: recording and detailed analysis of some 200 individual student-staff teaching conferences; individual interviews with each member of a student-faculty teaching unit; detailed questionnaires filled out periodically by each student; diaries in which one group of students made entries three days per week during the entire senior year; etc. These techniques revealed some clear differences (and some surprising lacks of expected differences) between the educational experiences of the specialty clinics control groups and the comprehensive medicine experimental groups.

To evaluate the effects of these differences in curricula ("what happened") the Dependent Variables Section developed procedures to assess in each student the following complex but important variables: newly attained medical knowledge; medical skill; medical attitudes; psychological knowledge; psychological skill; psychological attitudes; sociological knowledge; sociological skill; and sociological attitudes. Most important of all of the procedures employed by this Section was a new technique—one sure to be tried by research groups evaluating other professional or graduate curricular innovations—the Sound Film Interview. The student was presented a six-minute sound motion picture of a physician interviewing a new patient, after which the student opened a test booklet which provided him with certain factual medical information, such as laboratory findings, about the patient. On the basis of the information contained in the filmed interview and the test booklet, the student was required to answer multiple choice questions about the patient. The

questions required the student to apply his medical (psychological and sociological) knowledge and skill, and thus tested his ability in these areas. The technique also permitted objective evaluation of the student's skill in appraising a patient's intelligence, vocabulary level, and personality characteristics and, since 30 different patient-physician films were used, his capacity to perceive the individuality of each patient.

Because it (wisely) was expected that different students would vary in their perceptions of, reactions to, and effects derived from the experimental and control curricula, an Individual Attributes Section ("to whom the differences happened") studied such student variables as: initial aptitude for medical education; previous academic performance; attitudes; values; social interaction; needs; background information; self ratings; and peer ratings. The relations between these various classes of attributes and the student's performance on all the dependent variable tests were studied. That is, the measures of individual attributes were correlated with both the student's pretest performance on the dependent variable tests and the change in his performance from pretest to posttest.

While not a book which can be read in one sitting, since it includes some 350 tables of data and more than 50 figures, the report is well worth the time of those interested in the educational process and the intellectual-personality characteristics of different types of learners.

Some of the major findings were these. Compared to the control students in the Specialty Clinics' program, participation in the newly introduced General Medical Clinic: (1) *retarded* the development of the increasingly negative *attitudes* towards the theory of comprehensive medicine shown by senior students traditionally taught primarily system-and organ-specialization; (2) did *not* impair his acquisition of traditional medical knowledge and skill (despite the fact that the number of patients he saw was much smaller and despite the GMC student's *tenaciously* held opinion that it did (!)); (3) did *not* facilitate, contrary to expectations, his acquisition of greater psychological knowledge and skill; although (4) *individual attributes* in *both* curricular programs were found to be related to the acquisition of such psychological knowledge and skills; and (5) had the *negative* effect, as the senior year neared its end and graduation approached, of *increasing* the GMC senior student's *anxieties* regarding his tenaciously assumed, but otherwise not demonstrable (!), inadequate learning of traditional organic medicine—thus allegedly leaving him ill-prepared for his coming internship.

Interesting additional findings were: (1) the data do not support the assumption, made by some medical educators and almost all medical students, that it is necessary for a student, in order to acquire medical knowledge, to see a wide variety of patients with definite illnesses; instead of needing *extensive* experience, students benefit from *intensive* experience with fewer patients; (2) although the GMC's student's attitudes toward comprehensive medical care, in the abstract, became more positive, the GMC student did *not* become more interested in his patients—mainly because the psychological and social problems of patients were peripheral to the student's focal interest in learning organic medicine; (3) contrary to expectations, measures of a student's personality characteristics, attitudes, and values were essentially *unrelated* to medical staff evaluations; and (4) the medical faculty members *differed* widely among themselves in their appraisals of individual students, thus raising the specter that, if staff members in a single department cannot agree among themselves which students are good ones and which poor ones, staff evaluation cannot be used as a criterion for appraising a student's performance.

The book seems well worth the investment of time of psychologists, educators, and others interested in the process of education.

Where is human nature so weak as in the bookstore!

—Henry Ward Beecher

Saving the Self from Jeopardy

Robert Jay Lifton

Thought Reform and the Psychology of Totalism: A Study of "Brainwashing" in China. New York: W. W. Norton, 1961. Pp. x + 510. $6.75.

Reviewed by Robert Chin

The author, Dr. Lifton, is Research Associate in Psychiatry in Harvard University and an Associate of Harvard's East Asia Center. He has an MD from New York Medical College and received psychoanalytic training at the Boston Psychoanalytic Institute. The reviewer tells how he came to undertake the present research. Just now he is studying Japanese youth in Tokyo. The reviewer, Dr. Chin, is Professor of Psychology and Director of Research in the Human Relations Center of Boston University. He is the General Editor of the Journal of Social Issues *of SPSSI and co-author of a new Holt volume on planned change.*

—E. G. B.

In the cataclysmic changing of societies and of nations through the earthquake of revolution, we have a laboratory for seeing the "faults" of the psychological and cultural layers of personality, and, when a totalitarian regime uses a total push to exploit the inherent schisms in the individual and society, as in thought reform in Communist China, we must seek a conceptual grasp of these grinding processes. To do this we need to guard against the intrusion of our own fears of being manipulated and "brainwashed" by forces not under our control and of attitudes about Fu Manchu and the wily oriental.

Robert Jay Lifton brings the concepts and experiences of an ego psychiatrist to these important tasks. Psychiatric interviews with twenty-five western long-term sojourners in China and fifteen native Chinese, all forty of whom were extruded or escaped dur-

ing the Korean War, provide the data. He dramatically presents five illustrative cases of Westerners, plus six more who are studied as a group with follow-up interviews, and four Chinese subjects. None of the eleven Westerners is reported as being American. All show the psychic marks of thought reform.

Lifton's interest in thought reform came when he was an Air Force psychiatrist interviewing American prisoners of war in Korea. He went in 1954 to Hong Kong only to visit but stayed to interview escapees intensively on their experiences in thought reform. Upon his return to this country, he studied and investigated this problem for four years at Harvard, where he worked in psychiatry and in the East Asian Center. These interdisciplinary experiences he has used well. It is to be regretted that he makes no use or mention of the experimental researches on sensory deprivation and the processes of influence, attitude change, and persuasion.

Thought reform is viewed by the Chinese Communists, Lifton points out, as an essentially humanitarian and rehabilitative program, which is based on the twin Confucian notion of the perfectibility of man and of the rectification of his inner thoughts and which automatically, or at least eventually, leads to proper conduct and action. He makes the case that these assumptions, plus some Soviet ideology and practices, nurture the Chinese Communists' programs. It is true that the relations of "thought and action" have been central issues to Chinese leaders. Sun Yat-sen, Chiang Kai-shek and Mao Tze-tung have written papers on this topic. With the addition of the Marxist class-definition of thought and action, and an emphasis on the practice of using face-to-face groups, we have the necessary ingredients for "re-education." Are these "reformers" conscious manipulators? No evidence is presented by Lifton that they have a worked out scientific theory of behavior that guides their actions, that they claim or display any special acute observational sensitivity about people, or that any psychological research or researchers have been used in developing the practices. Like overcommitted and compulsive practitioners the world over, the Communists are interested in the results accomplished.

Lifton identifies a set of psychological steps involving confessions and guilt. He examines their emotional penetrations and the accompanying reactive changes in personality which, as rebirth occurs, bring about a new "self-role." In supplying a conceptual language for analyzing these processes, he employs almost exclusively the concepts of identity and totalism of Erikson (1954). Since Lifton does not define these promising and evocative terms, and since *CP* readers did not receive an exposition of them in the review of *Young Man Luther* (Norton, 1958; *CP,* July 1959, **4,** 201), we may paraphrase and quote Erikson here. The developmental task of the young adult is to achieve a sense of *who am I,* to separate himself from at least some of the emotional and behavioral patterns inherited from his past, and to struggle to achieve some sense of where he belongs and whither he is going. The role of ideology and values is central in this crisis. Totalism, a recourse when there is a loss of wholeness, is "a to be or not to be which makes every matter of differences a matter of mutually exclusive essences; every question mark a matter of forfeited existence; every error or oversight, eternal treason" (Erikson, 1958). Lifton's Western and Chinese cases show that identity is never completely achieved, can be shattered, reversed, or is precarious because of the suppressed but latent negative identity—aspects of self which one is not supposed to value or to become. His treatment implies that the identity crisis is never resolved in any person. Thus we can conclude that the ego theorists are confronted with the conceptual job of spelling out more precisely the types of reaction to the identity crisis and its vicissitudes and also of specifying the personal and social conditions under which each of them occurs.

His Chinese cases are vivid portrayals of the ragged emotional forces of filialism and family in the struggle by the contemporary Chinese personality to cope with recent political and social disasters. He could however, have advanced our understanding of personality and culture even more had he separated analytically the three aspects of the identity crisis—that natural to any young adult, that of Chinese youth culture's movement toward a modern culture in the last forty years, and that of political upheaval. He pays little attention to the supportive forces of peer groups, contemporary associates, and friends in each of these areas, although he does admit the role of peers specific to the process of thought reform.

Lifton generalizes the psychological themes of "ideological totalism," and he calls for "open personal change" as an alternative to the forces found in the coercive closed change of thought reform. Thus another voice is added to the medley speaking for the development of humanistic alternatives in bringing about changes in personal and social affairs.

If we rightly adhere to our rule (that we should occupy ourselves only with those subjects in reference to which the mind is capable of acquiring certain and indubitable knowledge), there will remain but few things to the study of which we can devote ourselves. There exists in the sciences hardly a single question upon which men of intellectual ability have not held different opinions. But whenever two men pass contrary judgment on the same thing, it is certain that one of the two is wrong. More than that, neither of them has the truth; for if one of them had a clear and precise insight into it, he could so exhibit it to his opponent as to end the discussion by compelling his conviction.

—R. Descartes

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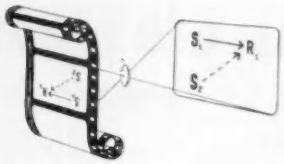

Primers Present Programing

Susan Meyer Markle, Lewis D. Eigen, and **P. Kenneth Komoski**

A Programed Primer on Programing. New York: Center for Programed Instruction, 1961. Pp. 1-25. $1.00 (10 or more, $ .50).

T.M.I.—Grolier

The Principles of Programed Learning. Albuquerque: Teaching Machines Incorporated, 1961. Pp. 1-13. (Single copies of this demonstration course free upon request).

Sam Epstein and Beryl Epstein

The First Book of Teaching Machines, New York: Franklin Watts, Inc., 1961. Pp. 1-50. $1.95.

Reviewed by GABRIEL M. DELLA-PIANA

The three co-authors of the Programed Primer, *all of whom are currently with the Center for Programed Instruction in New York City, are by now well known in the self-instructional field for production of self-instructional programs and research on programing. Markle, (who recently reviewed Blumenthal's* English 2600: CP, *Apr., 1961, 133-136) is perhaps best known for her vocabulary development program; Eigen (who reviewed Crowder's* The Arithmetic of Computers: CP, *Jan., 1961) for his* Sets, Relations and Functions—*a program in modern mathematics for high school students; and Komoski, President of CPI, for his remedial spelling program. James L. Evans, an unnamed author of the TMI-Grolier program, is also well known in the auto-instructional field as a collaborator of Robert Glaser and Lloyd E. Homme, and wrote one of the first doctoral dissertations devoted to an investigation of self-instructional variables. He is now Vice President of TMI. The Epsteins are a well known husband-and-wife writing team. Among the other "First Books" they have produced are* The First Book of Codes and Ciphers, The First Book of Measurements, *and* The First Book of Words. *The reviewer, Gabriel Della-Piana, is Assistant Professor of Educational Psychology at the University of Utah, where he teaches the diagnosis and treatment of remedial reading problems as well as psychology for secondary school teachers. He is currently conducting research on programed instruction, and has co-authored unpublished auto-instructional programs on counseling procedures and vocabulary building.*

IN his article on "The Science of Learning and The Art of Teaching" *(Harvard Educational Review, 1954),* B. F. Skinner wrote:

> There is a simple job to be done. The task can be stated in concrete terms. The necessary techniques are known. The equipment needed can easily be provided. Nothing stands in the way but cultural inertia. But what is more characteristic of America than an unwillingness to accept the traditional as inevitable?

Two vigorous publishers of carefully programed materials and a publisher of children's books who are overcoming cultural inertia are the Center for Programed Instruction (C.P.I.), Teaching Machines, Incorporated (now T.M.I.-Grolier), and Franklin Watts, Inc. C.P.I. is a "non-profit educational research and development organization, established in December, 1960, with the help of . . . the Carnegie Corporation of New York." The reviewer does not know whether the programers see their job as simple, but they are certainly going about the tasks of research and development with a great willingness. The C.P.I. *Primer* is an introduction to principles of programing, directed toward teachers as users rather than developers of programs. The T.M.I.-Grolier *Principles* is a brief program intended as an introduction for a different group—the interested layman. T.M.I. produced the first commercially available Skinnerian-type program, *Descriptive Statistics (CP,* March, 1961, **6,** 104-106). The *First Book* is not a learning program, but a conventional text in a series of "first books." Its authors have written this introductory book on teaching machines with a commendably controlled enthusiasm.

The review of the *Primer* is based on three sources of information: a study of the text, data and comments received in personal communications from the senior author, and data obtained in a tryout of the text by the reviewer. The current edition of the program is the product of three revisions made on the basis of responses from just a few subjects. Data on a more recent tryout by the authors on a group ($N = 48$) consisting mainly of teachers (mostly primary) will be referred to in the review. Reference to this recent tryout and to other information received in personal communications from the senior author will be indicated by "(PC)" unless otherwise specified. References to the reviewer's tryout, unless otherwise specified, will be indicated by "(R)". The reviewer administered the program to 21 students in an introductory educational psychology course (Summer, 1961), with the cooperation of Darrow D. McCarthy, the instructor, who also participated in the analysis of

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results. Students worked on the program at home after having begun it, under supervision, in class. The next class period books were collected and students were tested on 20 criterion frames (including 37 response blanks) selected (or adapted) from the program by the reviewer.

(Incidentally, as far as the reviewer could tell, this is the *first* learning program in which some grammatical variants of the term "program" are spelled consistently with one "m".) The program consists of 113 frames (or items) calling primarily for written, constructed responses (about 130 response blanks excluding multiple choice responses and a few other items), and required a mean time of 115 minutes to complete it (PC). In the reviewer's sample the mean time was identical. In items dealing with principles of effective programing, where there are conflicting data, the authors do a very good job generally of including qualifying phrases such as "there is no firm proof," "experimental tests . . . are needed," it is "an experimental question."

A study of the program itself gives us some clues concerning the objectives of the authors. Though criterion frames (those calling for the final desired responses under the appropriate stimulus control) are difficult to identify, it appears that the program is primarily calling for recall of definitions and principles in relation to terminology such as: reinforcement, programed instruction, cues or prompts, branching sequence, feedback, efficient and effective programs, fading, panel, active response, multiple choice response, and terminal and initial behavior.

The authors express, in their introduction, the ". . . hope that the 'student' who seriously works his way through this programed textbook will have gained insight into this teaching method, an insight which will be reflected in his own programs, if he chooses to teach this way" (page 1). This is rather vague language for a production growing out of modern behaviorism. Actually, in spite of the implications of the reference to the program's influence on one's "own programs," the present program is not intended to teach "how-to-program." The authors (PC) expect that in its final form (it is undergoing revision and a companion volume is in preparation) it "will enable teachers to look critically at programs, and accept good ones." Criterion tests for these intended behaviors in the revised and extended program will call for "analyses and reconstruction of bad programs," but no measurement of this nature has been reported for the present edition.

It would be desirable to see a more detailed statement of the intended terminal behaviors either in a separate list outside of the program or in a specification of which frames in the program are the criterion frames. Since the authors recommend such specification as a characteristic of effective programing (item 58) they ought to provide the information at least in a technical supplement.

THIS CPI program is structurally rather effective in terms of the authors' criterion of an effective program as (in part) one that "insures a high number of correct responses" within the program (item 54). The error rate within the total program was only 8% in the authors' study and 7% in the reviewer's study. Excluded from this analysis was item 47 (intentionally put in to demonstrate poor programing), the last three items (a closing pun), and other humorous or opinion type items.

Other characteristics of program structure recommended by the authors are fading (gradual withdrawal of prompts), branching, and active responding. Fading is recommended by the authors (item 46) as a technique for insuring correct responses and eventually getting correct responses without prompts. One of the interesting features of the *Primer* is the manner in which different kinds of prompts (verbal patterns, sentence structure, underlining, indicating number of spaces, indicating part of the answer) are introduced. In addition to describing a particular type of prompt, the program instructs the student to turn back to a previous item to see an example of a prompt of that type.

"Branching" is also introduced in an instructive and interesting manner. The student is asked for an opinion, and his response takes him down one of two branches. After that sequence he goes down the other branch. Then the technical term "branching" is introduced and the type of branching experienced by the subject is identified as "parallel" branching (as opposed to "remedial" branching where the student takes only one branch if he needs it.)

The term "branching sequence" is called for first under a highly prompted situation and then under diminishing prompt situations. However, the terms "parallel" and "remedial" are only called for under highy prompted situations. On criterion items written by the reviewer none of the group missed the response "branching sequence," 48% of the group missed the response "parallel branch" and 24% missed "remedial branch." This data supports the argument for "fading" as a characteristic of effective programs and points up a weakness in the program. Of course it also points up one of the problems in reviewing a program where the criterion behaviors are not specified. We don't really know whether the authors intended to get the responses "remedial branch" and "parallel branch" under the control of any particular stimuli. "Panel" is another term called for under a highly prompted situation (item 93) and never called for again as a written response. In the next two frames the term is used 8 times and thereafter 6 times to the end of the program. In a criterion item written by the reviewer, 38% of the sample missed the response "panel" (R). Here again, we do not know whether the authors were inconsistent with their own principles of programing or whether they were simply not interested in getting this technical term under the control of stimuli "without prompts." Because of the many other carefully faded items the reviewer suspects that the authors just weren't interested in teaching these terms. If this interpretation is correct, we might well raise the question as to why the program should be cluttered with technical terms not intended to be taught.

The principle of "active response" is also introduced in a fascinating way in items 29, 47, and 48. In item 29 the term "prompt" is introduced with "cue" as a synonym, but (intentionally) only the latter is actively called for. The term "prompts" is called for in item 47 and missed by 40% (PC) and 52% (R). Then, in item 48 the student is told about the desirability of "active responding" and his attention is called to the

fact that the term "prompt" was not actively called for earlier in the program. Incidentally, the term prompt (although it is used) is not actively called for again, but in a post-program criterion test only 14% missed the item (R).

There are a few minor errors in the program, such as the lack of clarity in the distinction between "effective" and "efficient" as a characterization of immediate reinforcement (items 16, 18, 19) and a simple rewording problem in item 22. These few items had almost no errors in the reviewer's sample, and indicate the need for not depending exclusively on conventional item-analysis data for program revision.

The fact that the detailed structural characteristics of a program may not be specified by the authors would probably be considered undesirable by some critics. Such specification is a desirable but not an essential characteristic of a good program. It is possible to conceive of effective programs that simply specify criterion behaviors and how well they are achieved by well defined norm groups.

As mentioned above, the authors speak of an effective program not only as one that "insures a high number of correct responses" but also as one that enables the student at the end to make correct response "without cues." (item 54). A word is in order concerning the notion of getting correct responses "without cues." The authors are not satisfied (PC) with their definition of a "cue" as "a technique . . . insuring a correct response" (item 29). The term "cue" as used in the program, is meant to be synonymous with B. F. Skinner's notion *(Verbal Behavior,* p. 253f) of a "prompt" as "supplementary stimulation." Thus, a criterion frame may be thought of as a potential discriminative stimulus (S_D) controlling the desired terminal behavior. At the beginning of a program, however, this stimulus (the criterion frame) is not sufficient to evoke the desired response. Supplementary stimulation (or prompts) may then be used to evoke the response. Slowly the prompts are faded until the learner is making the response "without prompts." The next paragraph reports some data relevant to the effectiveness of the program in getting correct responses "without prompts."

The error rate for the entire group (R) was only twelve percent. The error rate on these same items (in original form) within the program (including only 35 response blanks) was seven percent. (Error rate was computed as the ratio of "total number of wrong responses, summed over all subjects" to the "total number of correct responses possible summed over all subjects.") If these "criterion frames" can be taken as one measure of the expected terminal behavior, it appears that a group of prospective teachers, by going through the program, can learn this material rather well for immediate recall. The permanence of this learning, its use by the teacher in the analysis of programs and the reconstruction of bad programs, and the relative effectiveness of this method of programing instruction as compared with others, are matters for further study.

The cooperation of the authors of the *Programed Primer* in supplying the reviewer with information is a sample of what this reviewer sees as a rapidly developing readiness of publishers to provide "technical manuals" with information necessary for adequate evaluation of programs for specific uses.

This program will serve well as an introduction to programing for teachers. Even the limitations of the program and the "bad" items have been found "useful" for discussion purposes" by the authors and the reviewer. Perhaps the best indicator of the effectiveness (but not necessarily efficiency) of this program is that the TMI-Grolier programers have published "The Principles of Programed Learning," spelling programed with one "m."

A PERSONAL communication from the publishers indicates that TMI's *Principles* is intended to "present, explain, and exemplify five features of programed learning . . . [does not] attempt to build any technical vocabulary [and is primarily] designed for the layman who wants to learn more about programed learning, but probably does not want to construct a program." These objectives, of course, are stated primarily in terms of what the content of the program will be, rather than as what the layman will presumably be able to do upon completion of the program. However, an analysis of the program reveals criterion frames which shed a little more light on the objectives. Frame 38 is "Now see if you can list the five programing principles without hints:". The five principles of programing are: small steps, active responding, immediate confirmation, self-pacing, and student testing. Frame 59 calls for the rules under the prompt of five words which had previously been introduced as a mnemonic aid. Thus, the major objective as determined from criterion frames appears to be that of stating the rules. Other items calling for the rules under the stimulus of examples indicate that the objectives go beyond mere parroting of the rules under the stimulus of "Rule 1," "Rule 2," etc.

This is one program where the objectives are not difficult to determine by an analysis of the program. TMI-Grolier typically publishes information concerning the content, objectives, length, study time, and other characteristics of its programs. The program under review is intended simply as a "demonstration course" and does not include such descriptive data.

The major technique used by TMI to teach the five principles of programing is to give an example, state the rule and then call for the rule in a series of frames in which prompts are faded (rules are stated leaving out more and more words) until the entire rule is called for "without prompts." Each rule is called for with one or more of the preceding rules, but always in the same order. After the rules are called for in the first criterion frame (item 38), a series of items (39-48) call for the student to state the programing principle for examples which are to be identified as positive or negative instances of the five rules. Finally, in frames 50 to 58, five key words (read, write, check, advance, and record) are given as mnemonic aids for recalling the five rules.

The publishers claim to bring their programs generally to an average difficulty of five percent error responses; however, no data are given for this program. While some of our captured

subjects (graduate students) find the TMI program less interesting than the CPI program, it is the judgment of the reviewer that when given to the intended audience (laymen) the interest level would be high.

Although it is not a matter of great importance, Principle Five„ "The Principle of Student Testing" seems poorly named. The text reads "Revision of a program on the basis of student performance is called the Principle of Student Testing" (item 35). Since it is the program rather than the student that is tested it might be more appropriately named the "Principle of Program Testing." This of course is a matter of taste, since both syntactical forms have ample current usage.

While no data are available, there seems little doubt that an external criterion test would find most subjects well able to recall the five principles of programing. The five words used as a mnemonic key for the principles are so closely related to the process of going through a program that they would probably also be recalled and serve well as mnemonic aids, in spite of the fact that they are never actively called for as responses. It is questionable how much emphasis should be given to the article "the" in the title "The Principles of Programing"; however, this program appears to be a direct and effective introduction to the five principles named.

According to the information provided on the inside of the book's jacket, the *First Book of Teaching Machines* is the first book to read on the subject. Other books in the "First Book" series by the same publisher are recommended for grade levels ranging from "1 to 3" to grades "6 and up." No grade level is given on the jacket for the book under review. The reviewer analyzed five selections (roughly 100 words each taken about every tenth page) using the Dale-Chall readability formula (see *Educational Research Bulletin* **27,** 1948, 11-20, 28, 37-54). The text accompanying the 16 page insert of illustrations of teaching machines and learning programs was estimated at grades 9-10 for one selection and 11-12 for another. The other three selections analyzed were in the running text and were all in the grade 7-8 range.

The sixteen pages of illustrations begin with an excellent closeup of one of Pressey's early multiple choice machines with a candy reward mechanism. The reproductions vary considerably in the clarity of detail. Nine of the sixteen pages deal with TMI-Grolier machines and programs. Eight different machines are represented.

Technical terminology such as program, frame, multiple-choice, stimulus, response and reinforcement, are introduced gradually and at a very simple level. For example, the notion of reward is introduced early in the text. Then, on page 37 the authors write: "But if you answered his question correctly, he could say, 'You're right. You have done well.' He could give you the kind of reward that is most encouraging to students. That kind of reward or encouragement is what a psychologist means when he uses the word *reinforcement.*"

The need for "Teaching machines and learning programs" is developed beautifully in three sections from pages 28 to 34. First, "Learning with a private tutor" is illustrated, emphasizing that the major reasons for its effectiveness lie in allowing the student to move at his own rate and in diagnosing difficulty and providing aid at tough spots. Then, "Learning in a classroom" is discussed and illustrated, emphasizing how the essentials of good teaching may break down in a group situation (as well as how some things, such as class discussion and class projects, require a group). Finally, pointing out the tremendous task of education set by our increasing knowledge and increasing population, the authors conclude: "One solution to this serious problem, many educators and parents believe, may be found in a wide use of teaching machines and learning programs."

Since the authors don't indicate the purpose of their book, aside from the broad implications of the title, the reviewer's evaluation cannot include the extent to which they have achieved their purpose. The content of this book is accurate, although obviously the elaboration required for technical accuracy is limited by grade level and size of the text. The Crowder-type program is not named or illustrated, but is briefly described. The authors might have included a sample learning program for the student (something more extensive than the few frames used for illustrations). They might also have included a section for parents and teachers on cautions and hints and sources of information useful in making intelligent purchases of programs. This is a book that certainly will help the young reader to tell the difference between a teaching machine and a learning program, and to know (in simple terms) what it is that makes a "learning program" more effective than some other kinds of learning experiences. Junior high school students will probably take this book home to read, and probably will read it if they can get it away from their parents.

It has long been a complaint against mathematicians that they are hard to convince: but it is a far greater disqualification both for philosophy, and for the affairs of life, to be too easily convinced; to have too low a standard of proof. The only sound intellects are those which, in the first instance, set their standards of proof high. Practice in concrete affairs soon teaches them to make the necessary abatement: but they retain the consciousness, without which there is no sound practical reasoning, that in accepting inferior evidence because there is no better to be had, they do not by that acceptance raise it to completeness.

—J. S. Mill

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A symposium consisting of 26 eminent men was held in San Francisco under the auspices of the San Francisco Medical Center of the University of California and Continuing Education in Medicine and the Health Sciences. These men combine their knowledge, training, and experience in an investigation of the factors in the control of the mind and how they interact with each other. This text is that symposium and presents the ideas of outstanding men in the fields of science, psychology, sociology, history, religion, mass communication, and political science. Contributors include: Alexander Simon, Donald O. Hebb.

STATISTICAL PRINCIPLES IN EXPERIMENTAL DESIGN

By BENJAMIN WINER, Purdue University. *McGraw-Hill Series in Psychology.* Available February

This graduate-level text provides statisticians and experimental psychologists with basic principles used in the construction of experimental designs. Examined are designs found in current experimental literature and those with unique and potentially useful features. Their advantages and disadvantages are studied in detail. Examples are drawn from areas of experimental, industrial, and clinical psychology.

VOICE OF THE PEOPLE: Readings in Public Opinion and Propaganda

Edited by REO M. CHRISTENSON, Miami University, Ohio; and ROBERT O. McWILLIAMS, Eastern Michigan University. *McGraw-Hill Series in Political Science.* Just published

This readable anthology on public opinion and propaganda is designed for nonspecialists and students planning further study. The book includes: a discussion of the nature of public opinion; the dynamics of its process; and its role in a democracy. This is followed by a consideration of factors which influence or determine opinion. Propaganda principles, techniques, and effects plus types of propaganda are examined. Contributors include: Walter Lippman, Bertrand Russell, Richard M. Nixon, James Bryce, Arthur Schlesinger, Jr., George Gallup, Carl Sandburg.

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ON THE OTHER HAND

In this Department CP *invites discussion of reviews and of books reviewed. Here is the place for that kind of intellectual dissent that promotes progress in understanding. Let your criticism be* ad verbum, *not* ad hominem. *Seldom does a criticism merit more than half the space of the text criticized—never more than equal space and then only when the letter is interesting and well written.* CP *edits letters when it thinks they should be. Single-spaced letters will be returned for double-spacing.*

MORE ON SENIOR AND PRINCIPAL

Although we ordinarily don't write letters of protest to editors, we feel obliged to correct a misstatement of fact which appeared in the editorial preamble to the review of *Somatosensory Changes after Penetrating Brain Wounds in Man* by Semmes, Weinstein, Ghent, and Teuber *(CP,* 1961, **6,** 272-273). In the opening sentence of that preamble, *CP* says, flatly: "The senior author is really Dr. Teuber," How does *CP* know?

The senior author is Dr. Josephine Semmes (now at the National Institutes of Health). Your readers familiar with the literature will be aware of the fact that she has been publishing in the area of somesthesis for more than ten years. The order of the authors' names on the monograph reflects, as is customary, the relative contributions of each.

The three junior authors are understandably distressed by the injustice done to Dr. Semmes.

SIDNEY WEINSTEIN
Albert Einstein College of Medicine

LILA GHENT
George Washington University

HANS-LUKAS TEUBER
Massachusetts Institute of Technology

Note: The issue raised by this letter is thoroughly discussed by Boring in *CP,* November, 1961.

IN DEFENSE OF ZEN

If I am sad because a man of Arthur Koestler's gifts could write a book like *The Lotus and The Robot,* I am bewildered as to Roger Brown's review *(CP,* September, 1961, **6,** 291-292). The review itself appears to be defensive, anticipating criticisms of the book's value. For example, Brown writes, "He (Koestler) is sure to be rapped for ethnocentrism . . . Authorities on the Eastern religions and area specialists for Japan and India are certain to find that Koestler has not penetrated very far into their fields . . ." Just as 'explaining' does not 'explain away', so 'naming defects' does not do away with these defects of the book, which remain glaring. Brown's defense: "They (Koestler's reactions) are useful, I think, as predictors of our own reactions. Koestler seems to have more spiritual vocation than most of us, and he certainly has spent more time studying Yoga and Zen than we will do." I am, by no means, an authority on Yoga or Zen, but count me and, I am certain, many others, out of the editorial "we" of the quote.

Even a cursory reading of any book on Zen makes Koestler's response to the koans pathetic in the gross misunderstanding of their purpose. Contrary to Brown's prediction of the Zen Master's reaction to Koestler's koan answers, I would suspect the Master would eschew verbal and use harsh non-verbal behaviors. Moreover, Brown himself reveals an astonishing misunderstanding of the purpose of the koan when he gives a supposedly 'correct' answer to the koan, "Everybody has a place of birth. What is your place of birth?" There is no single 'correct' answer. Zen's primary attack is on the logic and language habits which create perceptual distortions of direct experience. The purpose of the koan (only one of the weapons in the Master's armamentarium, by the way) is to create insight into the distortions of reality which have been created by logic and language. The "answer" to the koan is immaterial in itself. The Master judges the answer, not by its adequacy, but by the effect the koan has created on the Zen student in terms of creating deeply felt insight. Koestler and Brown have failed on every count as Zen students, and they would be lucky to escape a Master's thrashing!

Yoga fares no better by the author or reviewer. One aspect of the genius of Yoga is the recognition of individual differences, so that the guru by intuition suggests one particular path (Yoga) or system to be emphasized by the particular student, depending on that student's personality. Hatha Yoga is apparently not for Koestler, but surely he knew there were many other systems. I would suspect that Koestler would be most comfortable with the Jnana Yoga, the path of intellectual aspirations. At least his squeamish stomach would not be threatened, if that is important. I might add that it is simply not true, as Brown asserts, "In the West, we hear only about a bowderlerized Yoga . . . Koestler gives us unexpurgated Yoga." I own only one book on Yoga, and that was bought three years ago in Minneapolis (diametric to Calcutta), at a conventional, open-shelved bookstore. This book contains not only the physiological exercises outlined in Brown's review, but more 'horrible' (if one is inclined to so valuate) rites such as *siva-saddhana,* the temporary animation of suitable corpses.

It is difficult not to make *ad hominem* arguments against both the author and reviewer, but one apparent contradiction in Koestler's orientation cannot be ignored. Brown first writes, "(Koestler) traveled in India and Japan in the mood 'of the pilgrim in sackcloth and ashes, anxious to prostrate himself at the guru's feet'." But two paragraphs later, "Koestler approached from the West with a mind full of Freud, Eddington, and Jeans, flying in on a British Viscount. As he descended its steps in Bombay, 'I had the sensation that a wet, smelly diaper was being wrapped around my head by some abominable joker'." Beware of verbalized humbleness!

A final comment about the reviewer's conclusion that Eastern mysticism has no contribution to make 'usable' by the West: How account, *inter alia,* for such diverse trends as the logical positivist's attack on language; existential philosophy; existential psychiatry; the inclusion of a section in the *Handbook of Psychiatry* on "Direct Experience," with one article entirely devoted to Zen; Fromm's book, *Zen Buddhism and Psychoanalysis;* Benoit's (French psychoanalyst) book, *The Supreme Doctrine;* and (to confine myself to a diverse list that could be multiplied manyfold) a

quote from the preceding review in *CP* to Brown's, by Maher, paraphrasing Magda Arnold, "Human experience is the primary datum of psychology . . . and when science deals with abstractions from these primary data, it is not dealing with reality." (*CP*, September, 1961, **6**, p. 289) Zen, by any other name, sounds the same, and I would conclude that Zen is not only usable, but *is* being used today in the West. For the most part, this is a result of independent discovery rather than cultural diffusion, but why ignore the original discoveries? Can it be that psychologists are using denial defenses about present cultural forces which are bringing the West closer to Eastern orientations? The psychiatrists and psychoanalysts do not appear to be excessively threatened. Why should we?

SHERMAN E. NELSON,
The Minneapolis Clinic of Psychiatry and Neurology

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Wiley BOOKS

UNCERTAINTY AND STRUCTURE AS PSYCHOLOGICAL CONCEPTS

By WENDELL R. GARNER, *The Johns Hopkins University.* In this book, the mathematics of information theory is used to develop the concepts of structure and uncertainty in such a way that they can be related to various problem areas in psychology. Structure is defined as the total relatedness of variables, and uncertainty is viewed as a prerequisite to structure. Two kinds of structure are identified: internal structure, which is the total relatedness within a subset of variables; and external structure, which is the relatedness between the subset and some variable or variables external to it in terms of their function. The author shows that structure is a primary factor in most human behavior and that its amount and form affect discrimination, learning, concept formation, and sequential behavior. He also demonstrates that internal and external structure are exchangeable in such a way that total structure remains constant. This fact has important implications for psychological research, since different aspects of behavior are not related in the same way to the two major kinds of structure. *1962. Approx. 376 pages. Prob. $8.95.**

CREATIVITY AND INTELLIGENCE

Explorations with Gifted Students

By JACOB W. GETZELS *and* PHILIP W. JACKSON, *both of The University of Chicago.* This book presents the results and conclusions of the most important research study ever conducted on high creative potential in school-age children and its relationship to high intelligence. It shows conclusively that high creativity and high intelligence (as measured by IQ tests) are by no means one and the same thing—and that highly creative children differ significantly from highly intelligent children with regard to personal values, aspirations, and family background. Empirical findings on creativity and intelligence are related to major theories of thinking and creativity, and case studies are presented which show the various types of highly creative and highly intelligent children. In addition, comparisons are made between children with high scores in tests of morality and adjustment and those with high scores in intelligence and creativity tests. The authors also explore the implications of their findings for education and for the theory and practice of evaluating individual intellectual ability. The methods used for measuring and assessing creativity are described in detail in the Appendix. *1962. 239 pages, $6.50**

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Studies of Development

By HERMAN A. WITKIN, RUTH B. DYK, HANNA F. FATERSON, DONALD R. GOODENOUGH, and STEPHEN A. KARP, *all of the State University of New York College of Medicine.* This book is concerned with deep-seated psychological patterns, encompassing the cognitive, personality, and social functioning of the individual. It gives particular attention to the origin and early development of these patterns in the family environment. A conceptual framework is developed for viewing interrelations between personality, cognitive, and social functioning in normal and abnormal states during development and adulthood. A large body of experimental and clinical evidence is adduced in support of this framework, so that the book represents an important contribution to the development of controlled procedures for studying personality. Equally important, the book brings into closer relation areas of individual functioning which are often treated as separate realms of investigation. An extensive bibliography and a comprehensive survey of the literature in the field is included. *1962. Approx. 448 pages. Prob. $7.95.*

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